# DISCOVERY

## A MONTHLY POPULAR JOURNAL OF KNOWLEDGE

EDITED BY A. S. RUSSELL, M.C., D.Sc.



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# Editorial Notes

A clear, uncoloured statement of what commercial aircraft can and cannot do has been recently given by Mr. Holt Thomas, and deserves attention. On the whole the prevailing opinion with regard to the commercial possibilities of aircraft is pessimistic. Yet during the war it looked sometimes as though aircraft were going to be everything in the future. Mr. Thomas's view lies between these extremes. He recognises that in this country the aeroplane cannot compete at present with existing forms of transport, but it may supplement them. It can with advantage carry mails, parcels, and passengers on urgent business, when part of the journey as ordinarily accomplished is over the sea. Thus an aeroplane can compete with train and boat in a journey from London to Paris or to Cork or Dublin, but not to Manchester or Edinburgh. The aeroplane travels, of course, twice as quickly as a train, but there is time lost in getting to and from the aerodromes at both ends, which reduces the overall speed of the journey considerably. Moreover, trains can, and aeroplanes usually do not, travel at night. In addition to all this there is the weather. Fog is the great nuisance in flying. Wind may help or hinder speed, but fog makes the pilot blind. Some kind of reliability of service is necessary before the business man will trust his messages or himself to the aeroplane. Yet, though we cannot abolish bad weather, we can partially conquer it by suitable organisation; and this is discussed.

Mr. Holt Thomas thinks that pleasure flying will never be very popular. It is too tedious, too uninteresting. Joy-rides in France seemed great fun. Does one become blasé after a bit? It seems so. One hears of owners selling their aeroplanes because flying bored them.

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A very interesting account of a journey in an unknown part of Arabia, Southern Najd, is given by Mr. Philby, of the Indian Civil Service, in the March number of *The Geographical Journal*. Mr. Philby is an authority on Arabia. He is the only living European who has crossed the peninsula from the Persian Gulf to the Red Sea. This account does not deal with this big journey, but with an "excursion" of three hundred miles out and three hundred home through a dangerous part of the country. From surveys made by Mr. Philby a map of this part of Arabia has been made. The account of a journey made by Palgrave, an Englishman, into Central Arabia in 1862 is criticised. Part of this man's account seems to be inaccurate, part purely imaginative.

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Quite recently an account has been given of a new way of making soap. Clay in the "colloidal form," when suitably prepared, may be satisfactorily substituted for a large proportion (up to about 50 per cent.) of the fatty acids in soap. (Matter may roughly be said to be in the "colloidal form" when it is in an extremely fine state of subdivision.) As clay is cheap, and soap relatively dear, the substitution of clay in this form for the fatty acids produces a notable reduction in cost. The clay is a real substitute for soap, and not an adulterant. Hot solutions of colloidal-clay soap form jellies on cooling, and thus this soap not only resembles ordinary soap in appearance, but in cleansing properties it is said to be even better than pure soap. Developments of this interesting discovery will be awaited with interest.

Mention was made in Mr. Darwin's article, in the second number, of the work of Mr. Aston at Cambridge on the purity of chemical elements. Mr. Aston first showed that the gaseous element neon is really composed of two gases the atomic weights of which are 20 and 22. He also showed that "pure" chlorine really consists of two gases of atomic weights 35 and 37. Nitrogen (atomic weight 14) and Carbon (atomic weight 12) appear to be "perfectly pure"—that is to say, *not* a mixture of two or more elements of almost identical properties, as are neon and chlorine. He has extended his results recently to other elements. Argon is found to consist of two gases of atomic weights 36 and 40; krypton of six gases, xenon of five gases, and mercury of a mixture of elements of atomic weights 204, 202 certainly, and of others about 197 to 200 in atomic weight. Helium (atomic weight 4) is found to be "perfectly pure." These results will be of extraordinary interest to all students of natural science. That a chemical element in its purest form should, all the time, be really a mixture of elements, which no *ordinary* chemical or physical process can separate, is a matter of great importance. In all cases which Mr. Aston has so far investigated, with the single exception of hydrogen, the atomic weights of these constituent elements, the "isotopes," are whole numbers.

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The great interest of Einstein's General Theory of Relativity may justify a brief note about his career. The following details were kindly supplied by Dr. E. Freundlich. Albert Einstein was born in March 1879, in Ulm in Würtemberg. His school years were spent in Munich. He early showed promise of an unusually brilliant future. He studied physics and mathematics at Zürich University from 1896 to 1900. On attaining his majority he changed his nationality by becoming a citizen of Zürich. During the next seven years he was engineer to the Patent Office, Bern. His first great achievement, the special Theory of Relativity, was enunciated in a paper read to the Berlin Academy of Sciences. It was soon followed by others dealing successively with the inertia of all forms of energy, the Brownian movement, and the Quantum Law of the emission and absorption of light. He became Professor Extraordinarius at Zürich in 1910, and later full professor at Prague, but returned to a permanent post in Zürich in the following year. The fundamental notions of his epoch-making theory, which has undermined the traditional conception of space, were evolved in 1907; but his attempts to solve the elusive problem of gravitation only began to show signs of success in 1912, when he recognised how much more simply gravitational phenomena could be interpreted by adopting non-Euclidean space. In 1914 he migrated to Berlin, where he has since remained. His general theory of relativity was made known to the Berlin Academy of Sciences in 1915, when he established the equations of the gravitational field, and succeeded in explaining mathematically the behaviour of the motion of Mercury's perihelion, which had long been a source of mystery to astronomers.

In addition to his scientific attainments, he is deeply devoted to the Zionist cause, and is a keen internationalist. He is also an accomplished violinist. During the war several of our men interned in Berlin found in him a good friend.

# The Date of the Nativity


## By W. M. Calder, M.A.

*Professor of Greek in the University of Manchester.*


It is well known that Christ was born some years before the date of the "Birth of Christ," which forms the starting-point of the Christian Era.[1] Luke in all probability intended to imply, and Matthew definitely states, that Christ was born before the death of King Herod, who ordered the "Massacre of the Innocents"; and we know for certain that King Herod died early in 4 B.C. Recent discovery—or discovery which would have been called "recent" in 1914—has thrown fresh light on the events which, in Luke's narrative, form the setting of the birth of Christ. We cannot yet assign the Nativity to a definite year, but many old difficulties have been cleared away, and the choice of a possible date has been restricted within narrow limits. At the invitation of the Editor, I will attempt to indicate briefly the character of the problem, and the bearing of the new evidence on its solution.

The words of Luke are as follows (ch. ii. 1–4): "Now it came to pass in those days, there went out a decree from Cæsar Augustus that all the [Roman] world should be enrolled. This was the first enrolment, made when Quirinius was governing Syria. And all went to enrol themselves, every one to his own city. And Joseph also went up from Galilee, out of the city of Nazareth, into Judæa, to the city of David, which is called Bethlehem, because he was of the house and family of David—to enrol himself with Mary his wife,

[1] How this traditional date was arrived at in the third and fourth centuries A.D. does not concern us here. Information on the matter may be sought under the articles Eusebius and Jerome in the *Encyclopædia Britannica* (ed. xi).

etc." And thus it came about that the child of Nazarene parents was born in Bethlehem.

This passage from Luke contains statements bearing on Roman Imperial administration, which at one time excited the surprise and moved the suspicion of historians. Mommsen, the greatest of nineteenth-century historians of the Roman Empire, writing in 1883,[1] went carefully into the literary and archæological evidence available at that date, and came to the conclusion that Luke must have grouped events belonging to different periods into a single year, and could not be writing accurate history. He concluded in particular that Quirinius could not have governed Syria before the death of Herod. Luke, as a matter of fact, does not state definitely that Christ was born before Herod's death, but Matthew does, and I personally accept the common view that Luke intends to imply it. Mommsen's opinion was almost canonical in matters of Roman history; and its effect may be measured by the fact that two learned and conservative writers in Hastings' *Dictionary of the Bible* (1900), one of them a first-rate authority on early Christian chronology, admit that, in his dating of the birth of Christ, Luke has been convicted of error. Other scholars took the opposite view, and new discovery has proved them right.

Combining Luke's narrative with the story as told by Matthew, we find four distinct statements which the Roman historian can test in the light of his knowledge of early Imperial administration.

1. That Christ was born in the reign of Herod (who died early in 4 B.C.).

2. That at the time of Christ's birth, the first census of the Roman Empire was being taken, by order of Augustus.

3. That this census was taken while Quirinius was governing the Roman province of Syria.

4. That to be enrolled, families had to report at a prescribed place, which in the case of Joseph was "his own city."

A number of inscriptions have been found, chiefly in Asia Minor, which bear on the date of Quirinius's governorship of Syria. A number of documents on papyrus have been found in Egypt which give a hint of the probable date of the first institution of the Roman census, and shed much light on its character and purpose. I am concerned here only with the inscriptions, but may refer first, and very briefly, to the papyri.

The papyri prove for Egypt—and their combination with literary evidence makes the same thing highly probable for all the Eastern part, at least, of the Roman Empire—that there was a periodic census, taken every fourteen years. The Roman Government taxed its subjects for the air they breathed, and boys became liable to this poll-tax at the age of fourteen; hence the period. A series of dated census-papers have been found belonging to the years 20, 34, 48, 62 A.D. If we extend the series upwards, we get the years A.D. 6 (when we know, from Acts v. 37, that a census was taken in Palestine) and 8 B.C. The latter year, as we shall see below, may very well be the year of the Nativity; but as the question of the earlier dates, and the local arrangements in connection with the census, is still canvassed by experts, we only lay claim to the presumption that a census, called by Luke "the first," was taken about 9–7 B.C.

The papyri also show that the census authorities ordered everyone to return to his city or village to be enrolled. Luke's statement regarding Joseph and Mary is an interesting record of an early and partial application of that principle of Roman Law which, in a later development, forbade certain classes to leave their home, tied the cultivator to the soil, and evolved the serf of mediæval Europe. But this is to travel beyond our subject.

Let us turn now to the inscriptions and the events with which they were concerned.

The mountainous country in the south of Asia Minor was notorious throughout the last century of the Roman Republic as a nest of pirates and brigands. In 102 the Romans annexed the plain of Cilicia, which formed a convenient base of operations against the pirates on the sea and the clans in the mountains. We read of more than one attempt to subdue the Taurus tribes, the most considerable being the campaign of Servilius Isauricus in 78–74 B.C., and the famous war of Pompey against the pirates in 66. In 63, Pompey annexed Syria to Rome; and Cilicia and Syria became a single army-command. The army in Syria had the double task of guarding the Euphrates frontier against the Parthians, and controlling the tribes that dwelt in the mountains to the north and north-west of Cilicia. To this responsible duty four legions were assigned; and this army we know to have been the only body of "regular troops" in the Asiatic provinces during the reign of Augustus. The Syrian legions were under the command of the governor of Syria, who, by the rules of the service, was required to have held the consulship (and usually had considerable experience of other provincial commands) before he became governor of Syria.

It is a matter of history that Quirinius (who had already distinguished himself as a soldier) was consul in 12 B.C. In the ordinary way, a Roman ex-consul who rose to the Syrian command took a round dozen of years to do it. Now, it is known that other Roman officials—we have their names and the dates of their tenure of office—governed Syria from 9 B.C. till after Herod's death in 4 B.C. Mommsen argued quite reasonably, on his evidence, that the first governorship of

[1] In his edition of the *Monumentum Ancyranum*.

Quirinius must be placed not earlier than 3–2 B.C. I say the "first" governorship, because Quirinius actually governed Syria twice, the second time being in A.D. 6, when the census mentioned in Acts v. 37 was taken in Palestine. What, then (so it was argued), would be more natural than that Luke, using a tradition that Christ was born during the taking of a census, while Herod was alive and Quirinius was governing Syria, should have confused the census of A.D. 6 with an earlier group of events, and produced a clumsy combination of two different occurrences, a census under Quirinius and a census in the lifetime of Herod? So it appeared to Mommsen.

The effect of the new evidence has been to bring out the true character of Quirinius's first governorship of Syria, and to prove that this governorship must have been earlier than 6 B.C., and in all probability covered the years 10–7 B.C.

Return now to the tribes on the north-west frontier of the double province of Syria and Cilicia. During the troubled years which intervened between Pompey's organisation of the East and the establishment of the Roman Empire (about 30 B.C.), the Romans attempted to control this region by means of a series of client kings, who were legally vassals of Rome, and were employed to keep the peace, to educate outlying districts in the ways of civilised life, and to train them for eventual absorption in the Roman Empire. One of the most efficient of these client kings was a Galatian called Amyntas, who at the beginning of the empire controlled a large part of the centre of Asia Minor, lying between the Roman territories in the west and north-west and those in the south-east. The main task of Amyntas was to free the road across Asia Minor, and to protect the central plains, from the inroads of the tribes in the southern mountains, and he attacked the tribes among their strongholds, had some successes, but was killed by one of the tribes, the Homanades, in 25 B.C.

Augustus now incorporated the kingdom of Amyntas in the Roman Empire, and undertook direct responsibility for the policing of the southern mountains. The literary tradition contains vague references to a war, fought by Quirinius, which brought peace at last to this turbulent region. The inscriptions, whose discovery extends from 1886 to 1914, illustrate the character of the war, and show that it was over in 6 B.C.

We have seen that Quirinius was consul in 12 B.C. He was a man of humble birth, to whom high office did not come in the ordinary course. Nor was the command of the Syrian legions held, in the ordinary way, immediately after the consulship. Quirinius must have been made consul expressly in order to qualify him for the command of the Syrian legions in the impending war, and his governorship of Syria had the character of a special mission. This is why we find a second governor in Syria at the same date: a governor of the ordinary type was required for the routine civil government of the province while Quirinius led the army on a distant, arduous, and lengthy campaign.

That the war was successful is proved by two pieces of evidence. An inscription now in the Lateran Museum of Christian Antiquities mentions that Quirinius received the Roman equivalent of an Earldom for the subjugation of a people—the name of the people was on a part of the inscription now broken away, but there is no doubt that it was the Homanades, who killed Amyntas, and who were specially aimed at in the campaign of Quirinius. A series of inscriptions, scattered throughout Pisidia, show that in 6 B.C. a number of garrison cities were being founded, guarding all the key positions in Pisidia and the country of the Homanades. These garrison cities were joined by a series of military roads provided with milestones, which give us the date.

Now it is obvious—especially to those who know the forbidding character of the Pisidian country—that those garrison cities could not have been founded, nor those roads built and measured by milestones, until the country was completely subdued. Quirinius's engineers could no more have constructed this elaborate system of roads—one of them through the very heart of the Homanadensian country—before the complete subjugation of the mountain tribes than General Wade could have built his fortresses and roads in the Scottish Highlands before Culloden. In point of fact the Pisidian mountaineers gave no further trouble to the Roman Empire for centuries.

The most recently discovered inscriptions actually mention the name of Quirinius, and show that he held an honorary magistracy in Pisidian Antioch—the G.H.Q. of the campaign—along with the governor of Galatia, in whose province the campaign was fought. This honorary magistracy—whose actual duties were carried out by an officer in Quirinius's army—had the effect of establishing a military dictatorship in Antioch, and was thus an essential step in the campaign. An argument too intricate to repeat here dates this magistracy about 9–7, and probably in 8 B.C. It may be regarded as proved beyond dispute by these inscriptions that Quirinius was fighting in Pisidia during at least two of the years 10–7, and that he was fighting there during his first governorship of Syria.

While Quirinius was fighting in Pisidia, Syria was being administered by an official called Sentius Saturninus, who held office in 8–7 B.C. Various ancient authorities mention Saturninus as the Syrian governor of that period, and Tertullian, a Christian who wrote about A.D. 200, and had access to Roman official sources, tacitly corrects Luke when he says that Christ was born

in the governorship of Saturninus, and that Saturninus took the census. Incidentally, we may note that Luke does not say that Quirinius took the census, but only that the census was taken while Quirinius was governor. It was different with the "second census" in A.D. 6, which is stated on an inscription to have been carried out, in Syria, by Quirinius during his (second) governorship. We are justified in concluding that Tertullian was using a good authority when he said that Saturninus took the "first census" in Syria—probably the Roman official lists. Why, then, does Luke date the Nativity by the name of Quirinius, whose governorship we have seen to have been of an exceptional and *ad hoc* character, rather than by the name of the ordinary Roman governor of the year?

Tertullian uses Roman official information, but Luke's story is caught from the lips of people in the East who remembered the events of the last decade before our era. What impressed itself on the memory of the contemporary Syrians and Cilicians was not the name of the humdrum civil governor, but that of the soldier who had fought a brilliant campaign on the North-west Frontier, had broken the power of the Pisidian robber chieftains, and freed the great trunk road along which trade, administration, Græco-Roman culture, and later on Christianity, moved from East to West and West to East.

It thus appears that the birth of Christ must be dated earlier than 6 B.C.; and that several convergent lines of argument point to a date 9–7, and probably 8 B.C.

NOTE.—In this short account many statements have necessarily been made briefly and dogmatically. The reader will get a good idea of the recent growth of knowledge in this subject if he compares the article on Quirinius in Hastings' *Dictionary of the Bible* (vol. iv, 1902), which was out of date before it was printed, with Ramsay's *Was Christ Born in Bethlehem?* (1898), and this again with Ramsay's more recent work, *The Bearing of Recent Discovery*, etc. (1915), chapters xviii to xxi. See also articles by Cheesman and by Ramsay in the *Journal of Roman Studies*, 1913–17. On the principle of law and administration exemplified in the journey of Joseph and Mary to Bethlehem see Zulueta in *Oxford Studies* (Vinogradoff, 1909), and the Russian scholar Rostowzew in his *Studien zur Geschichte des römischen Kolonates, passim.*

# The Transfusion of Blood


## By Geoffrey Keynes, M.D.

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THE idea of the transference of blood from the veins of one person to those of another is one that has always appealed to men's imaginations. It has seemed natural to suppose that the blood, circulating as it does so intimately in a man's body, must carry with it some of the characteristics of its owner. Old men have sought to regain their youth by having transfused into them the blood of boys; soldiers in France have refused even to let their lives be saved by having transfused into them the blood of Germans. It is improbable that transfusion in the ordinary sense was tried before the discovery by Harvey in 1616 of the circulation of the blood, but this discovery must have at once suggested the possibility of the direct transference of the blood from vein to vein, and various attempts were made. It is recorded in Sprat's *History of the Royal Society*, 1667, that Sir Christopher Wren was conducting "many new experiments, and chiefly that of *transfusing blood*, which the *Society* has prosecuted in sundry instances, that will probably end in extraordinary success." Wren's attempts did, however, end in failure, and it has been left to a later age to achieve the "extraordinary success" that was predicted two and a half centuries ago. To-day there are hundreds of men alive who have had the blood of others flowing in their veins, and, indeed, owe their lives to this very fact. But the blood-letting of war had lasted for two years before it became, chiefly through the American doctors in France, a common thing to save men's lives by the direct replacement of the blood which they had lost.

All the earlier attempts to transfuse blood were done by means of a direct flow of blood from an artery in the forearm of the "blood donor" through a tube into a vein in the arm of the recipient; but it was impossible to know how much blood had passed through the tube, and it was very difficult to prevent the blood from clotting in the tube and so blocking the flow of blood completely. An advance was made in 1892, when a method was devised of transferring blood from one person to another by means of a syringe, which was repeatedly filled from the donor's veins and emptied into those of the recipient. By this means the amount of blood could be measured, but the difficulty caused by the readinesss with which blood clots when it has left the body still remained. The physiology of the clotting of blood is even now, after a great amount of research has been done, an obscure and complicated subject. There is, however, definite knowledge of two of the conditions that are necessary in order that clotting may take place—firstly, contact of the blood with a rough surface which causes certain elements of the blood to disintegrate, and so initiates a chemical process resulting in the formation of a clot; and, secondly, the presence of the element calcium in an active form, since without this the chemical process cannot be completed. The first of these facts was met by the discovery that, if the blood be allowed to come into contact only with a smooth surface of paraffin wax, clotting was prevented

or delayed. If, therefore, the blood be allowed to run into a vessel coated on its inner surface with a thin layer of wax, and then be immediately run into the recipient's veins, a measured quantity of blood can be transferred with comparative ease. This method is still used at the present time, but it presents some technical difficulties, and even in practised hands is not absolutely certain of success. The last and greatest advance was made in 1913, when it was discovered in an American laboratory that sodium citrate, a salt of the acid present in lemons and other fruits, will combine with the calcium of the blood in such a way as to render it inert. Sodium citrate, if injected into the circulation in some quantity, has a very poisonous effect; but the amount of it that is necessary to combine with the blood calcium is so small that the mixture of blood and citrate solution is as beneficial to the recipient as if pure blood were given. Clotting is by this means entirely prevented, and the whole process of transfusion is rendered both simple and certain of success.

There still existed, however, a certain danger in blood transfusion, for it sometimes occurred that the recipient, so far from benefiting by the transfusion, sudddenly and unaccountably died; it was evident that some unknown quality in the blood remained yet to be discovered. These fatalities found their explanation, about the same time that the citrate method was introduced, in the discovery by another American observer that certain individual peculiarities existed in the bloods of different people. Half the volume of the blood is made up of large numbers of "corpuscles," each a minute disc, slightly concave on both sides and only seven-thousandths of a millimetre in diameter. They contain in their substance the red pigment, hæmoglobin, to which the colour of the blood is due, and by virtue of this substance they have the power of combining with the oxygen of the air, which they pick up in the lungs and convey through the circulatory system to the various parts of the body. Their function is thus indispensable for the maintenance of the life of the animal which they serve. It was now discovered that these corpuscles, which are of very delicate structure, are in some people destroyed by substances in the fluid part of the blood of others. Although this destruction would usually result only in the loss of the corpuscles of the transfused blood without any harm being done, yet occasionally so potent a poison was produced by the change that the consequences to the recipient of the blood were fatal. It was also found possible to classify people into four groups which exist in a given race of people in almost constant proportions. Of these, one group, the smallest (1 per cent.), cannot give blood to anyone except to people of their own group, though they can be given the blood of anyone. A second group, fortunately the largest (44 per cent.), are able to give their blood to anyone without ill effects, though they can only be given the blood of their own group. The two remaining groups, of intermediate frequency (15 per cent. and 40 per cent.), are mutually antagonistic, though their blood can be given to members of the first group mentioned and of their own group. A test for these groups can be easily made and is completed in a few minutes, so that it is now the practice to use as blood-donors people of the second group or of the same group as the recipient. The danger of death from incompatibility of bloods has thus been eliminated. It is believed that this grouping is not dependent on any fortuitous conditions, such as diseases that the individual may have suffered from, but to be due to differences of a chemical nature, which are inherited in a definite, though as yet undetermined, manner. The blood of a baby is found to have its characteristic group reaction as soon as it is born, and it does not necessarily belong to the same group as its mother.

It is difficult to find out accurately the total amount of blood circulating in the body, but physiologists have estimated it to be between seven and ten pints. It is still more difficult, for obvious reasons, to find out how much blood a man may lose and yet remain alive; but the body can accommodate itself by various mechanisms to differences in amount within fairly wide limits, and it is this power of accommodation that makes it possible to take from the donor as much as a pint and a half of blood not only without danger, but often without any effect at all, though some donors experience a transient faintness. Probably nearly half the total amount of blood can be lost without necessarily causing immediate death; but there can be no doubt that the loss of as much as three pints of blood will put a severe strain on the mechanism of accommodation, and to some individuals would be extremely dangerous. If a man has been severely wounded on a battle-field, his collapsed condition may have been brought about by several factors besides loss of blood. The wound may have given a very severe shock to the whole nervous system, the man may be cold and in need of food and drink, and he may in addition have lost blood in any amount up to three or four pints. As a result his heart may be exhausting itself in an attempt to force the remaining blood round a circulatory system which has become too big for it, and his lungs are failing to do their work of oxygenating the blood owing to the inadequacy of the supply that reaches them. The body will replace a certain amount of fluid at the expense of the organs, but this amount is limited.

Until the methods of blood transfusion became widely known, doctors sought to replace the fluid

that had been lost by injecting a solution of common salt in water into the circulation, and sometimes this served to tide the patient over a critical period. But often the effect was transient, the water that was injected being rapidly removed from the circulation and excreted. The immediate effect of blood transfusion upon a patient who is dying from loss of blood is one of the most gratifying sights that a surgeon can ever hope to see. The patient may have been literally within a few minutes of his death, but very soon after the beginning of the transfusion he begins to show signs of returning life. His breathing, from being a series of deep sighs, becomes normal, his pulse, previously very rapid and almost, if not quite, imperceptible, becomes slower and stronger, and his grey, drawn face regains its natural colour. The transfused blood, though probably not equal in amount to that which had been lost, has nevertheless served to turn the scale in favour of life, and the process has ensured that the patient shall, at any rate, not die from lack of blood. A blood transfusion carried out during the war was sometimes done under difficulties, and was apt to be a real race with death with a very brief margin of time; but however intense the effort, the result was nearly always an adequate reward. There was never any difficulty in obtaining volunteers for the office of blood donor. A number of men in hospital were usually tested for their blood groups before a battle, and the suitable ones chosen out. Most men took a genuine pleasure in saving the life of a comrade, and sometimes the rescue was effected under their very eyes. The donor was usually not ill-pleased to find afterwards that he was to get in addition three weeks' leave in England.

Towards the end of the war it was found that citrated blood could be kept unchanged for twenty-four hours or more, and in some hospitals the blood was drawn off from six or more donors when a battle was expected, and held in readiness; but even with this improvement it was still impossible during a long battle to carry out all the transfusions that were necessary. Physiologists in England therefore did much research in an attempt to discover a fluid which could be used as a substitute for blood and prepared beforehand in large quantities. It was necessary to find a non-poisonous solution which had approximately the same "viscosity," that is to say, the same physical properties, as blood, and would consequently not be removed so rapidly from the circulation as was the salt solution previously used. It was thought by some authorities that the corpuscles of the transfused blood were not important from the point of view of their special function as oxygen carriers, but that the maintenance of an adequate blood volume was the chief thing to be aimed at. Doubt has even been expressed whether the corpuscles of the transfused blood really carry out their proper functions in the recipient; but it has recently been shown that the visiting corpuscles can remain in the circulation for more than thirty days, and it is therefore almost certain that they can be as useful to the recipient as if his own corpuscles had been put back into his veins. Normally an adult person has approximately fifteen thousand million millions of corpuscles circulating in his body, and it is not difficult to believe that this number might be considerably reduced without ill effect. It was accordingly announced in 1918 that a satisfactory substitute for blood had been found in a specially prepared solution of gum-acacia, and it was used at once with high hopes of success; but it was found in France not altogether to justify the claims that had been made for it, and blood continued to be used whenever conditions permitted of it.

It is improbable that blood transfusion will ever be used on a large scale in civil practice, but it has already been found of great value for resuscitating patients who have undergone certain severe surgical operations; it has also been used with good effect in various diseases of the blood, such as pernicious anæmia, and has saved the lives of the unfortunate people known as "bleeders," whose blood, being deficient in the power of coagulation, continues to flow almost indefinitely after even a trivial injury. The injection of another blood gives the blood of the recipient, at any rate temporarily, an increased coagulability, so that the bleeder's wound is stanched. This effect is not spoilt by the admixture of citrate, although this is used as an anti-coagulant outside the body—a physiological paradox which has yet to be explained. Transfusion can also be used for cases of severe loss of blood due to accidental wounds. It is to be hoped that in hospitals it will always be possible to give a blood transfusion at very short notice; but the provision of blood donors is not so simple a matter in civil life as it was in the army. Already in America professional blood donors are to be found, who for a moderate fee are willing to sell a pint or more of blood several times a year; but Nature would not tolerate the letting of blood at intervals frequent enough to provide an adequate income, were this to be the donor's only means of livelihood. It is more probable that a suitable volunteer will have to be found for each case as the necessity arises.

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We hope to deal in the next issue with the following interesting books published by the Wireless Press:—*The Wireless Transmission of Photographs*, by Marcus J. Martin; and *Wireless Telegraphy and Telephony*, by H. M. Dowsett.

# The Concert of Europe in the Nineteenth Century

**By F. J. C. Hearnshaw, M.A., LL.D.**

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I

I TAKE it that one of the primary aims of DISCOVERY is to make new knowledge concerning the past, and new revelations concerning the present, available for the guidance of the future. Now, there can be no doubt that the hope of the future largely centres round the successful organisation and working of the League of Nations. It cannot, therefore, be without profit to ask what light history throws upon the difficulties and dangers which beset the path of those who attempt to constitute some form of International Government. The pages of history, particularly those that treat of the period subsequent to the break-up of Mediæval Christendom in the sixteenth century, contain many illuminating stories of efforts to co-ordinate mankind and establish perpetual peace among diverse sections of the human race. It is not the paper schemes of idealists like St. Pierre or Kant that are of supreme interest and importance. It is rather the practical attempts of statesmen and jurists to institute International Councils, to formulate an authoritative *jus gentium*, and to develop the machinery requisite for mediation and arbitration in disputes between peoples. To deal with these attempts at all completely would of course be a task far beyond the limits of a magazine article. But since the most significant of them are the latest, it may be profitable to give some special attention to these, and to ask what was the extent of the success and what were the causes of the failure of the Concert of Europe in the nineteenth century.

II

The Concert of Europe in the nineteenth century came into existence during the Napoleonic Wars. It was built up on the grand alliances which William Pitt and his successors organised for the deliverance of the Continent from the Corsican yoke. Its definite formulation may be assigned to the Treaty of Chaumont (March 10, 1814), the sixteenth article of which runs: "The present Treaty of Alliance having as its object the maintenance of the Balance of Power to secure the repose and independence of the Powers and to prevent the invasions which for so many years have devastated the world, the High Contracting Parties have agreed among themselves to extend its duration for twenty years from the date of signature, and they reserve the right of agreeing, if circumstances demand it, three years before its expiration, on its further prolongation." This compact marks a serious effort to establish a League of Nations, and to substitute for the tyranny of Napoleon an Amphictyonic Council of the Great Powers. The period which saw the most perfect manifestation of the operation of this embryonic Concert of Europe was that period of eight years, 1814 to 1822, which was marked by the overthrow of Napoleon and the re-settlement of Europe on the old dynastic lines. The tremendous perils through which the nations had passed, the awful sufferings which they had endured, the strenuous and combined efforts which they had been compelled to exert in order to regain their freedom, had given them a sense of community such as they had never had before. Their leading statesmen had come to know one another intimately; they had learned the magnitude of their common interests; they had become accustomed to act together. This was particularly the case with the monarchs and ministers of the four Great Powers who had concluded the Treaty of Chaumont—viz., Alexander I and Nesselrode of Russia, Francis II and Metternich of Austria, Frederick William III and Hardenberg of Prussia, Wellington and Castlereagh of Britain. Round this inner circle there gathered in close though less influential co-ordination the representatives of Spain, Portugal, Savoy, and Sweden. Outside, but seeking some pretext for admission, hovered Louis XVIII and Talleyrand, the diplomatic chiefs of chastened and subjugated France.

The first achievement of the Concert of Europe as constituted in 1814 was the negotiation and conclusion of the Treaty of Vienna. This is not the place in which to discuss the merits and defects of this famous reconstruction of Europe which determined the course of Continental politics for three-quarters of a century. Suffice it to say here that in the circumstances no settlement could possibly have been satisfactory and final, and therefore that the gravest defect of the Treaty was that it contained no provision for the reconsideration or revision of any of its terms. They were all regarded as sacrosanct and immutable. The conclusion of the Treaty in 1815, however, did not lead, as on all similar occasions previously, to the dispersal of the negotiators and the cessation of their Conferences. For one thing, the period of the discussions at Vienna had witnessed the escape of Napoleon from Elba, his return to France, and his inauguration of the Hundred Days' campaign. When the Final Act of Vienna was concluded (June 9, 1815), the Battle of Waterloo had yet to be fought. It was necessary, then, for monarchs and ministers to keep together in order to complete the new campaign, and in order to decide on the new terms of punishment to be imposed on turbulent and unrepentant France. For another thing, there was a general dread of "The

Revolution" in the minds of all potentates and politicians; a general fear lest the peril from France should break out again; a general consciousness that the settlement of 1815 would need a good deal of safeguarding. Hence, after the overthrow of Napoleon, the diplomatic crowd migrated from Vienna to Paris, and when there not only imposed upon France a pacification proportionate in severity to the offence which she had committed in supporting Napoleon in his mad adventure of 1815, but also took steps to perpetuate the Grand Alliance, which had thus completed its primary task. Two very different schemes were propounded in Paris for the constitution of the permanent European Concert. The first emanated from Alexander of Russia, who had arrived in Paris in a state of high religious exaltation owing to the influence of the Baroness von Krüdener and her evangelical company, into whose hands he had fallen in June 1815. On September 26 of that year he announced his plan for a Holy Alliance which should include all the Christian rulers of Europe, and according to which all should pledge themselves "to take for their sole guide the precepts of the Christian religion, to strengthen themselves every day more and more in the principles and exercise of the duties which the Divine Saviour has taught mankind." To this Holy Alliance all the Christian potentates of Europe gave their adherence, except the Pope and the Regent of England. No one, however, apart from Alexander of Russia, and possibly Frederick William of Prussia, took it seriously. It was from the first a mere "scrap of paper."[1] It remained entirely inoperative, and when Alexander died, ten years later, it faded out of diplomatic memory altogether. Lord Castlereagh had, when the terms were originally laid before him, described it as "a piece of sublime mysticism and nonsense." He was, all the same, alarmed at it, knowing to what numerous and varied interpretations the Christian religion lent itself, and not knowing what ambitious schemes of Western hegemony floated through the unbalanced mind of the formidable Tsar of all the Russias. Hence he not only prevented the Regent of England from doing more than express a vague and general approval of the Tsar's exalted sentiments, he also advanced another and entirely precise and conventional type of union, according to which the four Great Powers which had entered into the Treaty of Chaumont should continue to maintain their Quadruple Alliance for the limited and definite purpose of securing and ensuring the observance of the Treaties just concluded at Vienna and Paris. It was in this Quadruple Alliance of the four Governments (November 20, 1815) rather than in the shadowy Holy Alliance of the miscellaneous monarchs that the Concert of Europe embodied itself. As a support to its deliberations and resolutions, a combined army of British, Russian, Prussian, and Austrian troops, under the command of the Duke of Wellington, occupied the north-east frontier of France. As an instrument for the formation of its opinions and the expression of its will, the Ambassadors of the four Powers met daily at 11 a.m. at the British Embassy in Paris. Never before had the ideal of an international authority for Europe been so nearly realised. This condition of things continued for three years (1815–18). Then another Congress was held at Aix-la-Chapelle, which, *inter alia*, arranged for the disbanding of the international army, the evacuation of the occupied French territories, and the cessation of the quadruple control of French affairs. The members of the Concert dispersed, and Paris ceased to be the scene of their diplomatic operations.

## III

The harmony which, up to 1818, had in the main prevailed among the members of the Concert was after that date speedily broken. Britain was the first Power to strike a discordant note. At the Conference of Troppau (1820) she objected to the principle laid down by Metternich that in certain specified circumstances interference on the part of the Concert in the internal affairs of the Sovereign States of Europe would be justifiable; at the Conference of Laibach (1821) she protested against the intervention of the Concert in the domestic politics of Naples, where a revolution had overthrown the intolerable tyranny of the Bourbon Ferdinand; at the Conference of Verona (1822) she raised her voice against the proposed suppression of the constitutional government just set up in Spain, and against the proposed reduction of the Spanish American Colonies beneath the European yoke which they had repudiated. As in all three cases British objections and protests were ignored or overruled, Britain in 1823 withdrew from the Concert, and made her own settlements and arrangements with both the reconstituted European States and the revolted American Colonies. In respect of the latter she further came to an agreement with the United States of America in accordance with which the Monroe Doctrine was enunciated: both Powers resolved to prevent, by all necessary means, the interference of reactionary Europe in the development of progressive America. George Canning and John Quincy Adams combined to "call a New World into existence to redress the balance of the Old."

Whilst this schism between comparatively-liberal Britain and extremely-reactionary Austria, Prussia, and Russia was developing, another rift in the har-

[1] A German historian actually employs this expression, rendered so notorious by Bethmann-Hollweg in 1914. It is described by A. Stern, *Geschichte Europas*, vol. i, p. 41, as "ein wirkungsloses Blatt Papier."

mony of the Concert began to display itself. In 1821 the revolt of the Greeks against the Turks took place. Austria, under the leadership of Metternich, regarded this revolt as merely another instance of the rebellion of revolutionary subjects against the proper authority of their legitimate sovereign. She therefore encouraged the Sultan to crush the insurrection with all possible speed and all necessary severity. Russia, on the other hand, saw in the revolt the rising of an Orthodox Christian people against an infidel oppressor, and the passion of Russian sympathy, which in 1827 burst all the restraints of diplomacy, went out towards the struggling and suffering Greeks.

Before the complete independence of Greece had been secured by Russian intervention, still a third line of cleavage had manifested itself in the European Concert. In 1830 Belgium threw off the yoke of Holland which had been imposed upon her in 1815, thus violating one of the main stipulations of the sacrosanct Treaty of Vienna. Russia, Austria, and Prussia united in protesting against Belgian action, and in maintaining the authority of the settlement of 1815. Britain and with her France (under the new Orleanist monarchy), on the other hand, championed the Belgian claim to self-determination. Thus by 1830 the Great European Powers were torn by dissensions. Along the three different and conflicting lines of democracy, religion, and nationality, the Concert was divided into discordant groups. Only the fact that the three group-sections were not the same, and not even parallel, prevented the schisms from developing into open rupture. Long before 1830, in fact, the defects of the Concert of Europe, as established in 1814–15, had become glaringly evident. They were, first, that the Concert was merely a league of Governments: it was alien from the peoples of the European States; it was antagonistic to the popular principles of democracy and nationality. Secondly, that it lacked a communal consciousness and a general will: its members were particularist and selfish, each bent primarily on his own prosperity and aggrandisement. Thirdly, that it was committed to the maintenance of a thoroughly unsatisfactory settlement—viz., the treaty settlement of 1815, which embodied the compromises of diplomatists and the concessions of dynasts rather than the desires of the public opinion of the Continent. Fourthly, that it possessed no organs, and had provided no instruments, for the modification of the treaties of 1815, or for the settlement of new problems which might emerge from the changing circumstances of the new age then just begining. Finally, that the sphere of its operations was inadequately defined, so that, while Castlereagh, and after him Canning, insisted on restricting it to international relations, Metternich and the reactionaries were able to extend its activities to include interference in the domestic politics of the European States. With these grave defects in structure and mode of working, it is no matter of marvel that the Concert collapsed between 1822 and 1830. The wonder is that it managed to maintain any sort of symphony so long.

*(To be concluded next month)*

# The Measurement of Geological Time

## By Arthur Holmes, D.Sc., F.G.S.

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THE measurement of geological time depends on a comparison between the rate at which some process—geological, thermodynamic, radio-active, or astronomical—is going on at the present day, and the total effect of that process throughout the period of its activity. This is simply analogous to the fact that, if a body is moving with a known velocity, then the distance it travels gives a measure of the time taken. Average estimates of the rates of certain geological processes can be arrived at approximately; such, for example, as the rate of removal of detritus or soluble substances from the land surfaces, and the rate of deposition of sediments on the sea floor, or of the addition of salt to ocean waters. Knowing the total quantity of sediments that has been deposited since the hour-glass of denudation and deposition began its long career; or knowing the total quantity of salt that has accumulated in the seas since water first gathered in the ocean basins, it can be calculated to a first crude approximation how long these processes have been in operation. Thus, estimating the annual deposition of sediments at 9,000 million tons, and the total mass of sediments at 3,000,000 million million tons, the age of the oldest sediments works out by simple division at 330 million years. Similarly, estimating the total maximum thickness of all the sedimentary rocks at 335,000 feet, and the maximum average rate of present-day deposition at one foot in 900 years, the age is found by multiplication to be about 300 million years. However, these figures, although closely agreeing, must not be supposed to have any serious value, for they are clearly based on the assumption that the rate of sedimentation observed during a few decades represents the average rate for all geological time. To show that this assumption is not justified would require a

long and technical discussion. It may, however, be confidently stated that present-day rates are abnormally high; that denudation and sedimentation move in rhythmic cycles, sometimes rapidly, as now, but at other times much more slowly; that the rate of sedimentation is controlled not only by the supply of material, but also by the rate of relative subsidence of the sea floor; and, finally, that there are innumerable time-intervals unrepresented by layers of sediment in almost every part of the geological record. We may therefore be prepared for figures far in excess of even those crudely arrived at in the above estimates.

The early geologists were apt to be somewhat casual in their estimates of geological time, particularly as the latter were necessarily based more on sentiment than on quantitative data. More than half a century ago Sir William Thomson, afterwards Lord Kelvin, invaded the mists of speculation in which the subject was then enveloped, hoping to disperse them with the wand of thermodynamics. It is well known that, as the earth's crust is penetrated by bore-holes and mine-shafts, a steady increase of temperature is encountered. On the assumption that the temperature gradient of the earth was the result of simple cooling from a molten state, and knowing the average thermal properties of rocks, Kelvin was able to calculate within limits the time that must have elapsed since the consolidation of the crust took place. The first limits were 20 and 400 million years, with 100 million years as the most probable figure. Later, however, he reduced his limits to 20 and 40 million years, with a tendency to favour the shorter period. The long controversy which raged around these results is one of the familiar details of Victorian scientific history. Although a few geologists, impressed by Kelvin's authority and apparently unimpeachable reasoning, were willing to adapt themselves to the very severe limitations imposed, others, like Geikie and Goodchild, demanded far less restricted periods for the varied changes of life and scene revealed by the long succession of records preserved in the rocks. There was a hope, slight indeed at first, though afterwards fulfilled beyond all expectation, that some underlying flaw would be found in the thermodynamic arguments. It might have been pointed out that the latter ignored the great convection currents represented by the rise of granites and other products of igneous activity; but even had the calculations been modified to include such transfer of heat, the resulting estimates would have been little higher, if at all, than Kelvin's final maximum. In justice to Kelvin, however, it is only fair to state that he clearly realised that his estimates would fall to the ground if it could be shown that the earth contained within herself supplies of potential energy which, liberated as heat, would counterbalance the external loss by radiation. With the discovery of radio-activity the real flaw was at last detected, and it was found, contrary to Kelvin's assumption, that the earth is not a simply cooling body.

The examination of pitchblende by Mme. Curie led to the discovery of radium, and later it was found that radium is constantly being formed as one of a long series of elements which arise from the spontaneous atomic decay of uranium. During this disintegration radiations of three types are emitted by the unstable elements: $\alpha$-rays, consisting of positively charged atoms of helium; $\beta$-rays, identified with streams of (negatively charged) electrons; and $\gamma$-rays, of the same nature as X- or Röntgen-rays. After three atoms of helium have been discharged from the parent element, uranium, and its immediate descendants, radium is reached. Radium in turn decays, and as the rate of disintegration of each member of the family is proportional to the quantity of it which is present, an equilibrium is ultimately reached in which a constant ratio is established between the amount of uranium and that of each of the unstable daughter elements. The ratio of uranium to radium, for example, is 3,000,000 to 1. From the disintegration of radium, helium and a gas known as niton, or radium emanation, are evolved, and it is due to the easy detection and measurement of the emanation that small quantities of radium or uranium can be estimated. The emanation continues to break down, and ultimately a stable end-product which is chemically identical with lead is reached. When the whole family has fallen into equilibrium, the decay of one atom of uranium implies that of an atom of each of the unstable daughter elements, and the generation of eight atoms of helium and one of lead. The helium and lead, being stable, gradually accumulate in proportion as their ultimate parent, uranium, is destroyed. An atomic equation representing in two stages the total change is as follows:

$$\underset{\underbrace{238}_{238}}{\mathrm{U}} \rightarrow \underset{\underbrace{226 + 12}_{238}}{\mathrm{Ra} + 3\mathrm{He}} \rightarrow \underset{\underbrace{206 + 32}_{238}}{\mathrm{Pb} + 8\mathrm{He}}$$

It will be noticed that the atomic weight given for "Pb" is 206, whereas that of ordinary lead is 207·2. That this difference of atomic weight is real has been established beyond question by actual determinations on lead separated from uranium-bearing minerals in which the stable products of decay have accumulated through long geological periods. To this aspect of the subject we shall return for a more detailed discussion. Thorium is another parent radioactive-element, and its disintegration leads to the generation of helium at six stages with lead again as the probable end-product. The "lead" of the thorium family has apparently an

atomic weight of about 208, but unfortunately its genesis and accumulation are not yet as well understood as in the case of uranium-lead.

With this brief introduction to the nature and results of radio-active change, we may proceed to consider their bearing on the measurement of geological time. The helium atoms discharged as $\alpha$-rays are explosively emitted with very high velocities, in some atoms even approaching that of light. The atomic energy thus liberated is transferred to the molecules encountered by the helium projectile in its course, and appears as heat. In 1903 it was discovered that radium is constantly giving out heat; and evaluating all the contributions from its associated elements, we now know that the total generation of heat from the family represented by 1 gram of uranium amounts in a year to 0·7 calories. Similarly, from 1 gram of thorium in equilibrium the heat output amounts to 0·2 calories per annum. These amounts may appear to be trivial, but they suffice to provide the earth with such an embarrassingly large quantity of heat that it becomes necessary seriously to consider why the earth is not growing hotter. A simple calculation shows that a distribution of uranium throughout the earth amounting to only 5 parts in 100 million parts of rock would suffice to balance the loss of heat by radiation. The Hon. R. J. Strutt, now Lord Rayleigh, devised an accurate method for determining the uranium content of rocks, and his results and those of others show that the rocks contain 120 times more than is required. When to this the heat effect of thorium is added, it is found that, if the radio-active elements were uniformly distributed, then the amount of heat generated within the earth would be 300 times greater than the amount required to maintain a state of thermal balance. Since it is impossible to believe that the earth has been growing hotter at such a rate, it follows either that radio-active substances cease to give out energy in the deep interior of the earth, or else that they are restricted in their occurrence to the crustal rocks. Both hypotheses have their adherents; but whereas there is no experimental support for the former, there is a concordant body of evidence favouring the view that the deeper zones of the earth are free or practically so from these surprisingly energetic elements. The nature of this evidence need not here be discussed. It is sufficient to realise that the necessity of either hypothesis indicates how completely the foundations of the cooling argument have been destroyed. While it still appears probable that the earth's surface was originally in a state of aqueo-igneous fusion, it is certain that the cooling-down process has been extremely slow. The following figures show how the " age " is increased according to the proportion of radio-active heat admitted.

| Proportion of heat lost by radiation attributed to radiothermal energy. | Period of cooling required to reach the present temperature gradient. |
|---|---|
| 0 . . . . . | 20 million years |
| $\frac{1}{4}$ or 25 per cent. . . | 40 ,, ,, |
| $\frac{1}{2}$ ,, 50 ,, ,, . . | 120 ,, ,, |
| $\frac{2}{3}$ ,, 67 ,, ,, . . | 580 ,, ,, |
| $\frac{3}{4}$ ,, 75 ,, ,, . . | 1,600 ,, ,, |
| $\frac{4}{5}$ ,, 80 ,, ,, . . | 7,200 ,, ,, |

The discovery of radio-activity not only revealed with dramatic suddenness the unjustified restrictions which had been placed on geologists, but it led directly to the elaboration of the most elegant and refined method of measuring geological intervals of time that has yet been devised. Every fresh uranium-bearing mineral is now regarded as a natural chronometer, registering time by the atoms of helium and lead that are produced unceasingly within it year after year.

In 1910 Strutt directly measured the rate of formation of helium in the minerals uraninite and thorianite. He showed that 1 gram of uranium in equilibrium with all the members of its family generates 1 c.c. in 9 million years, and that for thorium the corresponding rate is 1 c.c. in 32 million years. In the case of the thorianite Strutt found that it originally contained 280 million times the amount of helium that could be generated in a single year, and therefore that the time required for its accumulation must also have been 280 million years. Before asserting that this figure is also the age of the mineral, three questions must be asked and answered :

(*a*) Was there any helium present in the mineral at the time of its crystallisation ?

(*b*) Has the rate of production of helium been uniform from year to year ?

(*c*) Has any helium escaped from the mineral during the period that has elapsed since its crystallisation.

The first question is answered by the fact that ordinary rocks and minerals contain only the slightest traces of helium, and that, if a radio-active mineral did contain a little of the gas as an original impurity, its amount would soon become quite negligible compared with the large amounts actually generated.

The second query presents little difficulty. No physical or chemical conditions that can be imposed on radio-active substances affect their behaviour in the slightest degree, and therefore the only variation in rate that can reasonably be considered is that due to the slow decay of the parent element, uranium. Since out of 7,000 million atoms of uranium only one disintegrates per year, it is clear that the correction to be applied is very slight.

Finally we must consider the possibility of leakage. It has been experimentally proved that as soon as a radio-active mineral is exposed to the air it begins to lose helium. On grinding for analysis more is lost,

and consequently it must lose a considerable amount before it is even collected in the field, especially when, as in the case of thorianite, it occurs in gravels which have been subject to the action of the weather for hundreds or perhaps thousands of years.

It is clear, therefore, that the helium now found in a mineral can be only a fraction, and sometimes only a small fraction, of the total amount which has been generated within it, and which alone could give a true estimate of its life-time. Returning to the thorianite investigated by Strutt, it will now be seen that it would be quite wrong to suppose that its age is 280 million years. The actual age must be considerably greater than that, and it should be carefully noticed that helium determinations never can provide data for more than a *minimum* estimate. All that the helium-ratio can tell us is that the age of the mineral to which it refers is greater than a certain minimum value. In the table below some of the minimum values which were obtained by Strutt are listed in order of geological age, and it will be noticed that in general they are less than half the corresponding values obtained from lead-ratios. To these, and the more reliable figures deduced from them, we may now turn our attention.

The original suggestion that lead is the stable end-product of the uranium family was made by Boltwood in 1905, and more recent discoveries have uniformly converged to demonstrate the correctness of his view. It is found that in fresh, primary uranium-bearing minerals of the same geological age, the amount of lead is closely proportional to that of uranium. Moreover, when series of minerals of different geological ages are compared, it is found that the ratio of lead to uranium increases with the geological age. Before the lead-ratio can be accepted as a reliable age-index, the questions discussed in the case of helium must, however, be answered satisfactorily.

If lead should have been originally present in uranium-mineral at the time of its formation, the age deduced will clearly be too high. Although lead is a negligible constituent in all the ordinary primary minerals of igneous rocks, it is possible that it may be present in certain radio-active minerals which are themselves associated with galena, the common ore of lead. Fortunately, however, it is possible to detect the presence of primary lead by means of an atomic weight estimation. If lead is wholly primary, its atomic weight is 207·1; if generated from uranium, the atomic weight is 206. Values between these figures indicate a mixture, and provide a means of determining the proportion of the total lead which is of radio-active origin.

The rate of production of lead in uranium minerals is not likely to have varied during their long history except for the gradual decrease due to the decay of the parent-element. As pointed out in connection with helium, no known agency has proved itself capable of affecting the rates of radio-active changes to the slightest detectable degree. From the rate of production of helium, a rate that is now accurately known from several independent methods of investigation, it is easy to calculate that 1 gram of uranium produces lead at the rate of 1 gram in 7,500 million years. Knowing the percentage U of uranium in a mineral, and that of lead, Pb, the age of a mineral to a first approximation is evidently $\frac{Pb}{U} \cdot 7{,}500$ million years. However, as the existing percentage of uranium is necessarily less than that originally present, a more accurate formula is $\frac{Pb}{U + 0{\cdot}575Pb} \cdot 7{,}500$ million years, where $(U + 0{\cdot}575Pb)$ is the average value of the uranium present during the life-history of the mineral. The figure taken from Pb may also have to be corrected by reference to an atomic weight estimation, but in practice none of these modifications make any serious difference to the order of the period of time finally arrived at.

So far the influence of thorium as a generator of lead has not been discussed. Although the evidence seems conclusive that lead is also the end-product of the thorium family, yet in thorium minerals of apparently identical geological age the ratio Pb/Th is often extremely variable. This inconsistency appears to be related to the fact that thorium minerals are usually very much altered, and in them neither the Pb/Th nor Pb/U ratios give reliable results. It is of course obvious that, if a mineral is altered, it has suffered chemical changes whereby the normal lead ratio is upset, for either introduction or elimination of lead may have taken place. The well-known radio-active minerals from Llano Co., Texas, are a warning example of the effect of alteration. Some of them are fresh and give a ratio of 0·17 for Pb/U, but others are secondary alteration products, and give ratios ranging up to 1·15.

In order, therefore, that age determinations should be reliable, it is essential that the minerals tested should be chemically stable, microscopically fresh, and of definitely known geological age. Further, since thorium minerals are frequently altered and give widely varying results, the minerals chosen should be those in which the parent radio-active element is predominantly uranium. If these conditions are fulfilled, the reliability of the experimental results can then be judged by two further criteria. The lead-ratios of a series of minerals of the same age should be concordant among themselves, and the atomic weight of lead separated from the minerals should be of the correct order.

Satisfactory minerals are available for dating three

of the geological periods fairly closely. Uraninite of Eocene (Lower Tertiary) age gives a ratio indicating that 70 million years have elapsed since it crystallised. For the Permo-Carboniferous igneous rocks of three different localities, ages of 300 to 340 million years are obtained. For the Middle Pre-Cambrian, a group of periods which includes at least two great epochs of igneous activity, ages varying from 900 to 1,120 million years have been deduced. The oldest mineral hitherto analysed appears to be a zircon from the Lower Pre-Cambrian of Canada. This gives an age of 1580 million years, but as it is based on a single analysis, it can only be considered to give an approximate estimate of the time that has elapsed. However, the figure is probably substantially correct, as it is supported by helium-ratios which, as usual, give less than half the period represented by the corresponding lead-ratio.

The data so far collected are summarised below in tabular form, and it will be noticed that in some cases the lead-ratio can be used, as in Mozambique, for determining the geological position of rocks which yield their age to no other method of investigation. There is, for example, no doubt that the zircons from Mozambique point to Middle and Lower Pre-Cambrian ages for the rocks from which they were respectively obtained. The correlation of the Pre-Cambrian formations in different parts of the world has long been one of the most difficult problems with which the geologist has been faced. The method of lead-ratios, however, has already done good service in leading the way towards a world-wide solution.

In conclusion it may be pointed out that the age of the earth is likely to be greater than, say, 1,600 million years, for the oldest known igneous rocks are themselves intrusive into sedimentary formations which in turn must have been derived from still older igneous rocks. The latter may possibly have been the original crust of the earth, but of this no certain trace has ever been detected. Now, as in Hutton's day, geologists can still find "no vestige of a beginning." Astronomical considerations, however, have shown in recent years that the new demands on geological time are not too high. The movement of the solar system across the void of space from its supposed birthplace in the Milky Way is a journey for which something approaching 3,000 million years is a dynamical necessity. Supporting this figure is an estimate by Dr. Harold Jeffreys of the age of the solar system. From a consideration of the present orbital elements of Mercury and their evolution, he finds that the requisite order of

GEOLOGICAL TIME SCALE, BASED ON THE LEAD-URANIUM RATIOS OF RADIO-ACTIVE MINERALS

(For comparison the ages calculated from helium-uranium ratios are given on the left. The much lower figures are, as explained in the text, due to the partial escape of helium from minerals before they can be analysed.)

| Millions of Years. | Minerals of which Helium-ratios are known. | Geological Periods. | Minerals of which Lead-ratios are known. | Millions of Years. |
|---|---|---|---|---|
| 1 | Zircon, Mayen, Eifel . . . . | Pleistocene | | |
| 2·5 | Zircon, Campbell Is., N. Zealand . | Pliocene | | |
| 6·3 | Zircon, Expailly, Auvergne . . | Miocene<br>Oligocene | Brannerite, Idaho . . . . . . | 30 |
| 31 | Hæmatite, Antrim . . . | Eocene | Uraninite, Gilpin Co., Colorado . . | 70 |
| | | Cretaceous<br>Jurassic<br>Triassic | | |
| | | Permian | Uraninite, Glastonbury, Connecticut . | 300 |
| 146 | Hæmatite, Forest of Dean . | Carboniferous | Uraninite and Zircon, N. and S. Carolina . | 330 |
| 147 | Zircon, N. Carolina . . | | Pitchblende, St. Ives, Cornwall . . | 340 |
| 145 | Hæmatite, Caen . . . . . | Devonian | Various, Brevig, S. Norway . . . | 360 |
| | | | (?) Uraninite, Branchville, Connecticut . | 390 |
| 209 | Zircon, Miask, Urals (?)[1] . . | Silurian<br>Ordovician<br>Cambrian | | |
| 307 | Thorianite, Ceylon (?) . . . | Upper Pre-Cambrian | (?) Uraninite, Morogoro, E. Africa . . | 660 |
| | | | | 900 |
| | | | Uraninite, Singar, India . . . . | 940 |
| | | | Uraninite, etc., Moss District, S. Norway . | 1,000 |
| | | | Gadolinite, etc., Ytterby, Sweden . . | 1,030 |
| 405 | Sphene, Arendal, S. Norway . | Middle Pre-Cambrian | (?) Zircon, Mozambique . . . . . | 1,120 |
| 449 | Sphene, Twedestrand, S. Norway . | | Uraninite, etc., Arendal, S. Norway . | 1,120 |
| | | | Uraninite, Villeneuve, Quebec . . | 1,120 |
| | | | Uraninite, Llano Co., Texas . . . | 1,120 |
| | | | Zircon, Ceylon . . . . . . | |
| 623 | Zircon, Renfrew Co., Canada . | Lower Pre-Cambrian | (?) Zircon, Mozambique . . . . . | 1,430 |
| 715 | Sphene, Renfrew Co., Canada . | | Zircon, Sebastopol, Canada . . . . | 1,580 |

[1] The sign (?) indicates that the geological age of the rocks in which the mineral occurs has not been unequivocally determined. In any such case the numerical age deduced from lead-ratios cannot be considered as a contribution to a geological time scale, but according to the place it occupies in the latter it may conversely be used as evidence of the geological age of the mineral and rock in question.

time is roughly 3,000 million years. Thus the earth recedes into an inconceivable remoteness far beyond the bounds of geological investigation, and there we must forsake her, and invite the astronomer, whose laboratory is the universe, to carry the story back still further.

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# The Architecture of the Next Twenty Years

**By W. S. Purchon, M.A., A.R.I.B.A.**

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In order to be in a position to appreciate the changes which are coming over the art of the architect in this country, it is necessary to glance at the development of architecture since the Gothic Period. In a brief article it is hardly possible to do more than mention a few of the major movements—the coming of the Italians during the reign of Henry VIII; the influence of the less refined work of Germany and the Low Countries in the days of Elizabeth and James I; the introduction of mature Italian design by Inigo Jones, and its vigorous development by our greatest architect, Sir Christopher Wren; the work of the amateurs of the eighteenth century; and the Greek Revival of the closing years of the eighteenth and the early part of the nineteenth centuries. During the development of the latter movement, the greatest monument of which is St. George's Hall, Liverpool, a return was made to the types of design which flourished in the Middle Ages, the rival schools indulging in the famous "Battle of the Styles." A further complication was a contemporaneous undercurrent of more or less pure Italian work.

During the third quarter of the nineteenth century George Devey, Norman Shaw, and others grasped the facts that we were not living in either Greece or Italy, and that the conditions of life had undergone serious changes since the time of Pericles, or even since the equally fascinating days of the Black Prince; and they attempted to base an architecture on the later traditional work of our own country and the special requirements of their own times. Largely to their efforts do we owe our acknowledged position as house designers.

The so-called "Queen Anne" style which resulted was subjected first to a coarsening process, and then to two further cross-currents—the "New Art" movement, which did some good by drawing increased attention to the importance of material; and a further attempt to get back to the fountain-head and to the use of Greek detail and forms as the alphabet of our architectural language. Possibly in sympathy with the *Entente Cordiale*, architectural forms which had received the sanction of Paris were also admitted by those who otherwise drank only from the Pierian spring. A better thing was, however, coming to us from the French capital—a greater ability to deal with the planning of buildings on a large scale.

And so the war found us: possessing considerable ability in the design of country houses, and some little trick of planning larger buildings on axial lines, and very interested indeed in the design of façades in the fashionable manner.

A note of warning had certainly been sounded by a few who saw that all was not well, and that salvation would not be found through the medium of ancient Greece with a dash of modern Paris, even if the façades were crowned with balustrades faintly reminiscent of the Union Jack. Whisperings of ferro-concrete were heard, ideas of Town Planning were drifting across the English Channel, and here and there a grandiose scheme for the embellishment of one or other of our cities was prepared, but it very rarely materialised. There was much talk of housing reform in the air, and a garden city and a few garden villages were actually built.

. . . . .

And now that the war has passed over us, leaving in its wake this somewhat peculiar peace, what is to become of architecture?

In the first place, the cost of building is now from two to three times as great as in pre-war days; secondly, the insistent demand is for houses, all the houses, and nothing but the houses (with a possible exception in favour of war-memorials); and, thirdly, the architects have been up against grim facts. Some of these facts are of a type which will hardly bear writing about, and in any case this is certainly not the place in which to write of them.

It seems clear, however, that those who are going to make the architecture of the next twenty or thirty years have passed far beyond the "pretty-pretty" stage. To them the art is a great and a serious one. They are neither the amateurs of the eighteenth century nor the archæologists of the first half of the nineteenth. Possibly they may base their work on the details of the buildings on the Athenian Acropolis, more probably on the sound and vigorous work of the architect of St. Paul's (work more in harmony with our climate and our usual building materials); but the important thing is that they will be practical artists, in touch with modern requirements and modern methods of construction.

Broadly speaking, the high cost of building work will lead naturally to a process of simplification; a con-

sideration of the cost of upkeep will result in soundness of building, and the feeling of these days through which we are passing will surely find expression in an austere beauty.

The richness of the East and the wealth of ornament of the fourteenth century may not be possible, while the vagaries of the days of Watteau and Meissonier——to take an extreme example—would be out of place; but without these there can, and there will, be great and noble building.

Much of the coming building will be—as has been stated—of a domestic character, and for this our traditions and training fit us well. Any weakness there was in our pre-war small house design was a tendency to fussiness, a tendency which is largely overcome by the conditions previously stated.

Important is the fact that the very vastness of our housing problem has led us to see that it must be tackled in a comprehensive manner. It has been clearly seen that the task of building the new homes and gradually getting rid of unhealthy dwellings necessarily raises the whole question of the right development of towns and cities. Great slum areas must be cleared as soon as sufficient new houses are ready, and thought is being exercised as to the right use of the sites. Traffic problems of increasing difficulty are being experienced in our larger cities, and the only sound method of tackling these and other difficulties is by means of the Civic Survey and the comprehensive town plan. The town—in some cases the town with its surrounding district, or in others a group of towns—is seen to be the unit; and the dictates of economy compel adequate investigation of existing conditions followed by systematic development, instead of the haphazard growth of pre-war times.

Thus in the case of housing it is clear that our extremity is really our great opportunity. As yet the new dwellings are coming all too slowly; but from the technical point of view the great scheme of putting right the housing of the people has been conceived on big and sound lines. Individually the houses will be more efficient than in the past, they will be pleasantly simple externally, and interest will be given by their disposition in short blocks on roads which have been carefully designed to suit the contours of the sites. And while the earlier sets of new houses are being erected, the schemes for dealing with the towns they adjoin are being matured.

Not only must provision be made for ample recreation spaces and allotments, but suitable sites must be reserved for a variety of public buildings which will be required at a later date. Already there is a serious shortage of schools, and large numbers will be required if education is to develop on sound lines. In all probability the school buildings—and possibly the hospitals—of the near future will be on less permanent lines than those of the past. This would be a sound step from a practical point of view, and it need not be regretted on æsthetic grounds, as it may well lead to interesting experiments with colour.

Many new public buildings of permanent character will, however, be required as the centres of population change with the development of our towns, and these buildings will give ample opportunities for the ex-service students who are now filling our schools of architecture.

There are difficulties ahead, but difficulties are more often than not a help to the able architect—Wren obviously revelled in the task of surmounting them—and it is the belief of the writer that our young men will rise to the occasion.

BOOKS RECOMMENDED.

Lethaby, *Architecture.* (Williams & Norgate, 2*s.*)

Statham, *A Short Critical History of Architecture.* (Batsford, 12*s.*)

Anderson and Spiers, *The Architecture of Greece and Rome.* (Batsford.)

F. Bond, *Gothic Architecture in England.* (Batsford, 34*s.*)

Anderson, *Italian Renaissance Architecture.* (Batsford, 16*s.*)

Ward, *Architecture of the Renaissance in France.* (Batsford, 40*s.*)

Gotch, *Early Renaissance Architecture in England.* (Batsford, 18*s.*)

Blomfield, *A Short History of Renaissance Architecture.* (Bell, 7*s.* 6*d.*)

R. Unwin, *Town Planning in Practice.* (Fisher Unwin, 42*s.*)

# Wireless Waves

## By Lt.-Col. C. G. Crawley, M.I.E.E.

In wireless signalling, energy is transferred from the sending station to the receiving station without any transference of matter, in an exactly similar manner to that in which light, heat, or sound is transmitted from one place to another.

In each case the energy is transferred by wave motions in the intervening medium.

Now, light and heat are transmitted from the sun to the earth not only through the atmosphere, but also through millions of miles of empty or vacuous space, so that the medium for the propagation of light and heat, whatever it is, is not a solid, a liquid, nor a gas, but must be something which is present in a vacuum.

This medium is called the Ether, and the laws of the propagation of energy by light, radiant heat, and wireless telegraphy can be proved if this ether is assumed to be present everywhere and to have certain definite properties.

The existence of a medium with these properties is therefore a fair assumption to make, unless, or until, some observed phenomenon requires its modification.

In this connection, for instance, it is possible that the result of recent observations of the sun's eclipse may require some modifications to be made in certain assumptions regarding the transference of energy by ether waves.

The energy which produces sound is also transmitted by wave motions of a medium, which in this case, however, is gaseous (as air), or solid or liquid.

The difference in the media in the cases of sound and light can be readily demonstrated by connecting up an electric bell and its battery under the receiver of an air-pump so that the space surrounding the bell, enclosed in the glass receiver, can be exhausted as far as possible of air. It will be found that, as the air becomes more and more rarefied, the sound of the bell decreases, eventually ceasing altogether when the receiver is exhausted of air. The hammer, however, can still be seen striking the bell, which shows that light is traversing the exhausted space, whereas sound is not.

The method by which energy can be transferred by wave motions in a medium can be seen by dropping a stone into a pool of water. The energy imparted to the water by the stone is radiated in the form of a circle of waves, the circle increasing in size, and the heights of the waves becoming smaller, until they reach the edges of the pool, or die away altogether. Now, if some corks are floating in the pool, the energy imparted to the water by the stone, and propagated by the waves, will cause the corks to bob up and down, thus showing that the little particles of water, which are in this case the medium, merely oscillate up and down, and by doing so, convey energy in the form of waves, the corks nearer the original disturbance receiving more energy than those farther away.

The stone, by disturbing the water at one place, has thus transferred energy to other places by waves in the medium, the water, without any transference of matter taking place.

Similarly, if one end of a piece of rope is fixed, and the other end is rapidly moved up and down by hand, energy is transferred by wave motions in the rope from the hand to the place where the rope is attached without any matter being transferred from the one end of the rope to the other.

In both these cases the wave motion by which energy is transferred is produced by oscillation of the little particles which make up the medium—that is, the water or the rope, up and down at right angles to the line of propagation of the energy. A wave length is the distance separating the crests of adjacent waves, and it will be apparent, by observing the motions of the rope, that the more rapid the oscillation the shorter are the wave lengths, and vice versa.

It is in just such a manner that light and heat are transmitted from, say, the sun to the earth, except that the oscillations of the medium, in this case the ether, take place not only in one direction, but in all directions at right angles to the line of propagation.

If the oscillations of the ether are at certain rates, they affect the eye and produce the sensation of light; if at certain other rates, they are received not as light, but as heat.

If the ether is set in oscillation at very much slower rates than can be detected as light or heat, the ether waves produced are those which are used for communication by Wireless Telegraphy. These wireless waves are often called Hertzian Waves, after Hertz, a German physicist, who in 1878 published the results of his experiments on the production and properties of these waves, results which confirmed theoretical deductions published in this country by Clark Maxwell in 1864.

The transference of energy by all these ether waves, light, heat, and wireless, takes place at the same rate, viz. 186,000 miles a second; but the rates of oscillation of the ether on which depend the lengths of the waves differ very much in each case.

The highest known rates of oscillation in the ether produce Röngten or X rays, then come the actinic rays, which are used in photography, and then the visible light rays, at rates of oscillation from about 1,200 billions a second for violet light, down through indigo, blue, green, yellow, and orange, to about 600 billions a second for red. A combination of all these colour rays produces white light, i.e. sunlight. Below these rates of oscillation are the heat rays, and then the rates of oscillation, about 3 millions to 15,000 a second, which produce the Hertzian waves now used for wireless signalling.

These wireless waves are therefore very similar to heat or light waves, and travel at the same speed, the main difference being that the rates of oscillation of the ether which produce them, though so exceedingly rapid, are far too slow to produce the sensation of heat or light; in other words, these wireless waves can be neither felt nor seen. Neither, of course, can they be heard, as they have no connection whatever with audible sounds, which are produced, not by ether oscillations, but by air oscillations, acting on the drum of the ear.

The speed of travel of ether waves, 186,000 miles a second, is so great as to be, for all practical purposes, instantaneous so far as propagation over the earth is concerned; though when the stupendous distances dealt with in astronomy are considered, this speed is, of course, far from instantaneous. For example, it takes some eight minutes for light from the sun, and about four years for light from the nearest star, to reach the earth.

Signalling by an unscreened flashing lamp may be

taken as an example of signalling by light waves, flashes of short and long duration representing the dots and dashes of the Morse Code. These flashes consist of trains of ether waves which radiate in all directions at a speed of 186,000 miles a second, the oscillations getting weaker and weaker as the distance from the lamp increases, until at last they have dissipated their energy and die away altogether. The eye can be used to receive these signals, and the message in the Morse Code can be read simultaneously from all directions by any number of people within range, provided that there are no screening effects caused by opaque bodies intervening between their eyes and the lamp.

Wireless signals are sent in a very similar manner by short and long trains, or series of trains, of ether waves which travel outwards from the sending station in all directions at this same speed of 186,000 miles a second. These trains of waves actuate, as dots and dashes, according to their duration, the wireless receiving instruments at any number of stations which are within range, and are adjusted to respond to the particular wave length used.

These long ether waves are able to pass readily through bodies which are opaque to light or heat waves, unless such bodies consist of, or contain, conductors of electricity, in which case the waves produce currents of electricity in the conductors, and so part with some of their energy.

Screening effects in the case of wireless waves are therefore very different from those which obtain in the case of light waves; for example, buildings, etc., opaque to light waves, may present no obstacle whatever to the passage of wireless waves; even mountains, and indeed the obstacle caused by the earth itself, do not affect these waves in the same way as they affect light waves.

For instance, there is no difficulty in signalling by wireless waves across the Atlantic, whereas it is impossible to signal over distances of more than a comparatively few miles by light waves, owing to the earth imposing, due to the curvature of its surface, an impenetrable obstacle to such waves. In the case of the Atlantic Ocean this obstacle would represent a mountain some hundreds of miles in height, and the waves do not actually pass through this obstacle, but may be considered as gliding over the surface. Like light waves they are subject to absorption, to reflection, and to other forms of bending, as refraction and diffraction, on account of which mountains or other high land close to a wireless station may produce serious screening in that direction, whereas the same obstacle at a distance may have very little or even no appreciable effect. Short waves are much more affected by this land screening than are long waves; but in any case it is advisable to avoid siting a wireless station close to higher ground in a direction towards which communication is required.

Absorption and bending effects in the atmosphere account for the fact that signals, especially when using short waves, can be transmitted to much greater distances by night than by day, and that these night ranges may vary considerably during the same night, and from one night to another.

For signalling by wireless, the sending station does not transmit a number of ether waves of various lengths at the same time, as is the case when signalling by light, but makes use of one wave length only for that particular transmission.

All wireless communication is not, however, carried out on the same wave length, as if it were the receiving stations would receive a jumble up of all the messages being sent on that wave length from stations within range, instead of the particular message which it was desired to receive. When the reception of a message is thus interfered with by other messages being sent at the same time, the message is said to be "jammed." This jamming of a message may also be caused by stray ether disturbances in the atmosphere itself. These disturbances are called "atmospherics" or "strays," and are specially strong when thunder-storms are in the vicinity, lightning itself causing very violent atmospheric disturbances. Atmospherics often cause great interference with wireless communications, and in tropical countries are sometimes so strong as to prevent any communication at all for hours at a time. They are strongest as a rule at night and in the afternoon. In more temperate latitudes, as in this country, atmospherics are usually at their worst during the night in summer-time, but are seldom so troublesome as those experienced in warmer climates.

The actual choice of the wave length will depend on various considerations—for example, high-power long-range stations cannot be arranged to transmit short waves, and low-power short-range stations, such as those in ships, cannot be arranged to transmit long waves.

All commercial ship and coast stations are fitted so as to be able to transmit on wave lengths of 600 and 300 metres, the intention being that communication, if jammed on the one, may possibly be established on the other.

The S.O.S., or distress signal, is always sent on the 600 metres wave, as this is the wave length for which the receiving apparatus in all ships is normally adjusted.

BOOKS RECOMMENDED

*Waves and Ripples in Water, Air, and Aether.* By J. A. Fleming, D.Sc., F.R.S. (S.P.C.K., 7*s.* 6*d.*)

*Textbook on Wireless Telegraphy. Vol. I, General Theory and Practice.* By Rupert Stanley, B.A., Temp. Major R.E. (Longmans, 15*s.* net.)

# The Antiquity of Man in East Anglia

## By E. N. Fallaize, B.A.

WITHIN the last few years investigation of the deposits of gravel and clay of Glacial and Pre-Glacial Age in the neighbourhood of Ipswich has furnished a considerable body of evidence which throws fresh light on the early history of man in Britain, although, owing to a variety of causes, it has not perhaps received the attention that it deserves. One of the most interesting of these discoveries was recently described at a meeting of the Royal Anthropological Institute, when Mr. J. Reid Moir, by whom the greater part of these investigations has been carried out, exhibited a number of flint implements obtained from the Chalky Boulder Clay in the neighbourhood of Ipswich. The implements were found, during a period extending over some twelve years, in pits excavated for the purpose of brick-making. They do not occur in any great quantity, but Mr. Reid Moir has secured a number sufficient to furnish a definite idea of the type, and with some probability of the stage of culture to which they belong. So far as the form and technique of these implements go they are to be assigned to the Mousterian type, so-called from the site of Le Moustier in France where implements of this type were first discovered.

It will perhaps be necessary at this point to explain that in the classification of the French archæologists, which is generally accepted, the Palæolithic Age is divided into six periods or phases of culture, each named after a characteristic site in France. The implements belonging to two of these periods or phases——the Chellean and the Acheulean—are of the "Drift" or "River-drift" type, so called because they are found in the drift of gravels carried down and deposited on terraces on the sides of valleys when the river-beds were in process of formation. The implements of the remaining periods—Mousterian, Aurignacian, Solutrian, and Magdalenian—belong to the "Cave" type or series, as they have, in France at least, usually been discovered in caves which have served as the dwelling-places of Palæolithic peoples. In technique the distinction between the "Drift" and the "Cave" series of implements lies in the fact that, while the "Drift" implement has been shaped by striking flakes from a block of flint, the "Cave" type—e.g. the implements from Le Moustier with which the implements from the Suffolk Chalky Boulder Clay are comparable—are fashioned from the flakes themselves which have been struck from a block of flint or "core."

The great interest of Mr. Moir's discovery lies in the fact that implements of Mousterian type should be found in the Boulder Clay. Geologists have, as a rule, taken it as axiomatic that the earliest implement-bearing stratum is the Boulder Clay, and it will be remembered that the investigations made by the Committee of the British Association at Hoxne in Suffolk, of which such distinguished authorities as the late Sir John Evans and the late Mr. Clement Reid were members, showed that on that site, at least, flint implements of the "Drift" type were later than the Boulder Clay, while the borings carried out by the Committee furnish evidence of two warm periods intervening between the gravels in which the implements were found and the Boulder Clay, which is a deposit of Glacial Age. On the evidence from Hoxne, implements of the "Drift" type would therefore appear to be of Post-Glacial Age.

In France the relative age of the different classes of Palæolithic implements has been established with some precision. Implements of Le Moustier type come third in point of time, following the two series belonging to the "Drift" type from Chelles and St. Acheul. In England the relation of the "Drift" and the Le Moustier types is not so clear; but on the basis of analogy, it might be presumed that "Drift" implements precede the other types. It may be noted in parenthesis that Mr. Reid Moir, in his recent publication "Pre-Palæolithic Man," after a careful analysis of the technique of Palæolithic implements, arrives at the conclusion that the "Drift" implement made from a core precedes the Mousterian implement, which was made from a flake. Archæologists are therefore faced with this difficulty—that implements apparently belonging to a later stage of culture have been found in a deposit which, in another locality, precedes implements of an earlier type by a very considerable period of time.

If, therefore, Mr. Moir's implements belong to the Mousterian phase of culture—they are certainly of Mousterian type, and some exhibit a form of patination usually considered to be characteristic of the Le Moustier period—and no doubt can be raised as to their occurrence in Boulder Clay, the question resolves itself almost entirely into one of the geological evidence. Prof. J. E. Marr, F.R.S., who has examined the pits, states that the Boulder Clay is *in situ*. It would appear, therefore, that the question before the geologist is whether the Chalky Boulder Clay is everywhere of the same age, as is generally stated. If it is, is it possible that the evidence obtained by the Committee at Hoxne was untrustworthy, and that it was misled as to the true character of the deposit identified as Boulder Clay? Some geologists would maintain that the finding of Mousterian implements in Chalky Boulder Clay is evidence of late date, and that this view is supported by the palæontologists, who hold that an examination of a continuous series of the fauna of this period shows no signs of an alternation of warm

and cold phases, but that the strata with remains of a cold period are always on top, and therefore latest in date. A careful study and re-examination of the question of the Boulder Clay generally and of the deposits at Hoxne would appear to be necessary before any certain conclusion is possible.

# Mistakes[1]

THIS is a straightforward book with only one difficult word, and that is in the title. It contains an exposition of Dr. Freud's views on the psychology of lapses and of mistakes. It is a work of interest to everybody, because it deals with mistakes which we all make. In this book, which is the best known of all Freud's writings, an attempt is made to explain some of the little slips we make in writing and speaking, why it is we forget proper names and foreign words, why we try to conceal some of our memories, and why, when meaning to do a thing in one way, we do it, almost in spite of ourselves, in another. Everyone is familiar with these occurrences. What is the cause of them ?

An ordinary person would imagine that the main cause of all our daily slips and errors is that we are tired. If we sign the name of the friend written to at the end of our letter to him, instead of our own, we ascribe the slip to the fact that we are not sufficiently alert ; we are mind-wandering. Freud's view of the matter is, however, different. The mistake, in his view, is symbolic of hidden motives which are at work in the mind of the man who is making the mistake. The slip gives him away. To anyone trained to see it reveals more than the man meant to reveal. The slip enables the psychologist to brush aside the open intention of the man as being false, and to get at the true underlying motive.

An example taken from p. 102 of the book may help to make this point clear :

" A wealthy but not very generous host invited his friends to an evening dance. Everything went well until about 11.30 p.m., when there was an intermission, presumably for supper. To the great disappointment of most of the guests there was no supper ; instead, they were regaled with thin sandwiches and lemonade. As it was close to election day the conversation centred on the different candidates ; and as the discussion grew warmer, one of the guests, an ardent admirer of the Progressive Party candidate, remarked to the host : 'You may say what you please about Teddy, but there is one thing—he can always be relied upon ; he always gives you a *square meal*,' wishing to say *square deal*. The assembled guests burst into a roar of laughter. . . ." Here the error of speech revealed the speaker's mind with full comic effect. He had no intention of being openly rude. He was so in spite of himself.

An example of an action erroneously carried out will now be quoted from p. 179 :

" The use of keys is a fertile source of occurrences of this kind, of which two examples may be given. If I am disturbed in the midst of some engrossing work at home by having to go to the hospital to carry out some routine work, I am very apt to find myself trying to open the door of my laboratory there with the key of my desk at home, although the two keys are quite unlike each other. The mistake unconsciously demonstrates where I would be at the moment.

" Some years ago I was acting in a subordinate position at a certain institution, the front door of which was kept locked, so that it was necessary to ring for admission. On several occasions I found myself making serious attempts to open the door with my house key. Each one of the permanent visiting staff, of which I aspired to be a member, was provided with a key to avoid the trouble of having to wait at the door. My mistake thus expressed the desire to be on a similar footing and to be quite 'at home' there."

Freud's point, then, is that these slips are not accidental at all, but that in every case they can be explained ; and usually the explanation can be given in terms of the secret feelings and wishes of the person making the mistake.

This book is full of anecdotes about slips made by people, and each of these is ingeniously analysed by the author along the lines stated above. Some of the explanations will appear to the reader to be somewhat far-fetched, but, of course, that need not prevent them being the true ones. What the book does impress upon one is that we attribute far too many errors to accidental and extraneous causes. As regards our own mistakes, if only we have the wit to examine ourselves, we may usually find some explanation which lies not outside but within ourselves.

To say that every mistake is symptomatic of unconscious secret wishes is, of course, going too far. If, for example, in writing to a lady I refer to her letter as " your precious letter " instead of " your previous letter," this need not necessarily indicate any hidden state of feeling which I may have for the lady. It happens that *c* and *v* are next one another on my typewriter, and I happen merely to have struck the

[1] *Psychopathology of Everyday Life*. By Prof. S. Freud. Sixth Impression. (Fisher Unwin, 12*s*. 6*d*. net.)

wrong letter. It is useless to assert in reply that something in me made me strike the *c* instead of the *v*, because I find that as often as I strike *c* for *v* I strike *b*, which is the letter on the other side of *v*; and as that gives a meaningless word when I try to type "previous," I conclude that my mistake is purely accidental, and not in the least deliberate.

An extreme case—it is not given in this book—is that of the billposter who fell off his ladder into a bucket of whitewash. If we assert that this was not accidental, we are led to believe that the billposter had a secret desire either (*a*) to end his life, or (*b*) to whitewash himself or his character. Now, the billposter fell because the rung of the ladder broke, and we need not assert that something in the hidden consciousness of the man knew this, and made him stand on the weakened rung.

On the other hand, many a far-fetched explanation may be a true one. Only this morning a friend, in describing Mr. Maynard Keynes' book on the *Economics of the Peace*, said: "It is very good, but it has one or two *ships*." He meant *slips*. Now, Freud would say there is a real explanation for this, and explanation there is. For this friend of mine, when speaking of the book, was thinking of the author's position, which is Fellow of King's, and *kings* was closely associated in his mind with *ships* by those lines of Lewis Carroll (quoted by him erroneously):

> "Shoes and ships and sealing-wax
> And cabbages and kings."

It may seem a long way round, but it is nevertheless, as my friend agrees, the correct solution of the difficulty.

So what we say of this book is that it is very stimulating and very interesting. A reader need not swallow Freud's theories whole. But there is a lot in them, and a good deal of it is great fun.

P. K. FORBES.

## The Idolatry of Science[1]

MR. COLERIDGE has written a very amusing book; indeed, since Mr. Stephen Leacock attacked the Classics in his comic article on "Homer and Humbug" we have rarely read so amusing an "attack." Mr. Coleridge's general thesis is that, on the whole, science has had a bad influence on the world, and that really it is high time that some responsible person or body of men protested against the blind worship which it receives everywhere. It simply isn't

[1] *The Idolatry of Science.* By the Hon. Stephen Coleridge. (John Lane, 3*s*. 6*d*. net.)

[*Continued on p.* 120

good enough. By science he means something which is "entirely distinct from, and opposite to, poetry, letters, oratory, history, and philosophy; something that has no relation to, or connection with, the emotions, or with the character of man; something wholly unconnected with conduct; something with which the principles of right and wrong have no concern." By scientists he singles out men like Darwin, Newton, Lord Merchiston (the inventor of logarithms), Lord Lister, and Dr. Flexner. These people and others, and their views, are discussed at length.

It is well that Mr. Coleridge is an open and courageous enemy of science. He does not tolerate it, wishing in his heart that it had never existed. He does not content himself by damning the thing with faint or with patronising praise. He hates it. He sees in it the remorseless enemy of mankind, a something which destroys simplicity, beauty, and gentleness, something which restores barbarism under the mask of civilisation. He wishes it had never come into being. His attitude at times is rather like that of Arnold of Rugby. "Rather than have physical science the principal thing in my son's mind," wrote he to a friend, "I would gladly have him think that the sun went round the earth, and that the stars were so many spangles set in the bright blue firmament." And Hegel too adopted this position. In attacking Newton's theory of the movement of planets according to universal gravitation, that philosopher said that the planets were not pulled this way or that way like so many stones, *but that they move of themselves in their orbits like the blessed gods!* But Mr. Coleridge goes further than Arnold and Hegel, further, too, than the poets of two hundred years ago, who poked fun at science as it then existed. He has no use for science at all. He longs for the good old days before Messrs. Watt and Stephenson (as he calls them) got busy on the steam-engine, before factories polluted our air, before telephones took away the last vestige of our privacy, and before those infernal doctors began inoculating their patients, and experimenting with animals.

Says Mr. Coleridge:

> "How am I advantaged as a man, and as one who loves his country, by getting to Edinburgh from London in eight hours, having seen nothing; instead of getting there in three or four days, and seeing all the loveliness of the countryside, the peasants happily working in the fields, the sweet, unconscious beauty of the villages, the parks and comely mansions with their stately gates on the old high-road, the venerable churches with their ivy-covered rectories hard by, the quaint red brick almshouses, founded ages ago by pious benefactors, with their placid old inmates sitting out in the sun, all eloquent of the blessed repose of the quiet life?"

Hear, hear! we say to this. It is all very delightful, and so far we agree with every word he says. It calls to mind Mr. Chesterton's mediæval peasant, or Tom Pinch's ride to London in *Martin Chuzzlewit*, or Goldsmith's "Sweet Auburn, loveliest village of the plain." Yet the remedy for avoiding the rush to Edinburgh is so simple that it must have occurred to Mr. Coleridge. Go by motor-car. And here, I confess, I am wrong. Mr. Coleridge is not against *everything* scientific. He excepts the bicycle and the motor-car. They are all right, but who on earth wants to know about conics and chemistry, or about wireless and medicine? The folk in the good old days got on without them: why cannot we? Is it any use telling immortal souls that centrobaric dispositions are kinetically symmetrical? Are we really any the better for rushing about in tubes, or being shot up in elevators, or making cotton pants by machinery? The author thinks not, and he is entitled to his opinions. Of course, there is no harm in knowing something about astronomy or how to build bridges and bore tunnels, and how the common-suction pump works, and how many blue beans make five. This, however, is not wisdom. Why put on airs because we know these things? Anyone who is awake and not mentally deficient can acquire these facts in a few hours. But wisdom—ah! wisdom is a plant of slower growth.

Mr. Coleridge is most interesting and most amusing when he is attacking. Now, we all love an attack. It doesn't matter specially what it is that is being attacked, so long as it is an attack, *and we are the spectators.* How annoying it is to find that the street fight has concluded just a few moments before our arrival! Mr. Coleridge's special "stunt" is to poke fun at the apparent weaknesses of men of science. The editor of a scientific journal prints two absolutely contradictory statements on opposite pages of his magazine, and Mr. Coleridge does not let us forget it; a meteorologist says, in an unguarded moment, that "the sun itself does not give out heat," and we are treated to a humorous commentary thereon; a chemist describes the preparation of ortho-cyano-benzyl-hexamethylene-tetraminium chloride, and Mr. Coleridge administers to him a dialectic upper-cut for not spelling it more shortly.

He thinks, too, that scientists are arrogant. Boyle, Cavendish, Russel Wallace, Faraday, Clerk Maxwell, Silvanus Thompson arrogant! Yes.

> "They are all illustrious and world famous, they pelt each other with degrees and diplomas, the whole country rings with their mutual hosannas, and the fountains of honour play upon them like a fire-engine on a conflagration."

He considers that the title of "Professor," except when assumed by conjurers, jugglers, and tumblers, stamps a man as narrow, prejudicial, inaccurate, ignorant, and dangerous.

It is a little difficult at times to know whether Mr. Coleridge is serious or not. One would almost imagine from the way he writes that he disapproves of inoculation and of vivisection. We like especially his amusing mock picture of typhoid germs. It is so good that we are sorry that he has not prepared more illustrations for us. May we help him here? On p. 64 the author says, "Sir Joseph Lister was raised to the peerage in a halo of antiseptic spray." What a grand opportunity that is for the nimble pencil of Mr. Heath Robinson! Again, on p. 56 we are told that the scientist

"will not argue or listen to argument; he brings forward no reasonable evidence, and will not attend to reasonable evidence brought before him by others. He is a man of science, he peddles in a laboratory, he asserts, and when he speaks let no dog bark!"

An illustration of this passage by Mr. Max Beerbohm would absolutely make the second edition. The scientist on the left of the picture carefully brushing aside every argument but his own, and busily peddling; on the right six dogs straining at the leash, and being just restrained from barking by the warning finger of a boy. What a chance for the artist! It is too good to miss.

It is a pity that Mr. Coleridge allows his views to run away with him at times. With many Englishmen a specialist training leads to what seems to us a narrow view. One man sees the cause of war and misery in "the lawyers," another in "the politicians," a third in "the clericals," a fourth in "the international Jew financier." Mr. Coleridge thinks it is science. But these are all bogey-men. With his main point that the teaching of science without an accompanying training in other subjects, in character, and above all in religion, is dangerous, we agree. But what sensible man or woman would deny this? Yet humour, whether conscious or unconscious, is so rare these days that we welcome it wherever we see it.

A. S. RUSSELL.

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PROFESSOR J. B. WATSON, of the Johns Hopkins University, is the author of a new book on Psychology. It is written for University students, and the subject is treated in a thoroughly scientific manner. The ordinary reader may consider parts of it stiff, but the psychologist will find in it a fresh and unconventional treatment of his subject.

# Reviews of Books

*Silvanus Phillips Thompson, D.Sc., LL.D., F.R.S.: His Life and Letters.* By JANE SMEAL THOMPSON and HELEN G. THOMPSON, B.Sc. (Fisher Unwin, 21*s.* net.)

Professor Silvanus Thompson was born in 1851 and died in 1916. Those sixty-five years of his were crowded ones, for he was a man of restless activity. He made a name for himself first and foremost as a science teacher. He made important contributions to our knowledge of electricity, optics, and electro-technology. He was the author of several very clearly written textbooks. He was the biographer of Faraday and of Lord Kelvin. He was, in addition, a man of varied tastes and hobbies. He was a member of the Society of Friends. He loved the scientific literature of the sixteenth and seventeenth centuries. He drew in black-and-white, and painted in water-colours. A man of varied interests, the chief of which was in Science.

We are indebted for this interesting biography to his wife and his daughter. They have given us a happy and intimate account of the professor. The material on which the book is based has been well used, and the style and the setting forth is good. Very wisely the authoresses have arranged their material, not in strict chronological order, but according to subject. This helps to make a book easy to refer to, and easy to read. In the case of scientific biography it enables the non-technical reader to read what interests him most. The more technical details, being grouped in one or two chapters, may, if desired, be skipped.

Silvanus Thompson was a familiar figure in scientific circles in London, because from 1885 till his death he was the principal of Finsbury Technical College. The Royal Society and the Royal Institution saw much of him, and popular audiences at the latter heard with delight his expositions of scientific knowledge. He was an exceedingly good lecturer. No trouble was too much for him, no experiment too difficult. It was all worth while if it would serve to make his lectures clearer. Better understood, better remembered.

He began research at Bristol, where in 1876 he had been appointed Lecturer in Physics at the newly founded University College, and his first paper on phenomena connected with induced electric sparks was printed in the same year in the *Philosophical Magazine.* The long list of published papers which is given in the appendix of the biography testify to his originality, his versatility, and to his capacity for work. Like Lord Kelvin, he had that wonderful power of taking the most extraordinary interest in anything that was new. If he had an interest in a thing, it was a keen interest, an enthusiasm. All his research was good work, though actually, as it happened, none of it became epoch-making. Once, indeed, he came near to a very big thing, but someone else arrived independently at substantially the same results a little before him. In 1895 the discovery of X-rays by Professor Röntgen of Würzburg made a tremendous sensation. A few months later, Thompson, while working with X-rays, found that uranium nitrate had an action on a photographic plate in circumstances in which X-rays had not. This was something new, and he wrote immediately to Stokes, then President of the Royal Society, about it. Alas! Becquerel of Paris had anticipated him by a few weeks. This was the first discovery in the science which later became known as radio-activity.

In 1881 Thompson's book *Elementary Lessons in Magnetism and Electricity* was published. This was a very good textbook. It has had a wide sale, and has served more than one generation of students. Two years later he wrote an account of Reis, the inventor of the telephone. The invention of the telephone is usually ascribed to Dr. Graham Bell, but in this book Thompson showed that, sixteen years before Bell's invention was made known, a Philipp Reis of Frankfort made an instrument for the express purpose of transmitting speech by electricity, and called it the "Telephon."

Thompson was very keen that honour of priority of invention should go where honour was due, and several controversies on this point in which he took part, especially in connection with the invention of the dynamo and of wireless telegraphy, are touched on in the work under review.

In 1896 appeared *Light Visible and Invisible,* a popular and well-written book. Two years later appeared his life of Faraday. This is considered to be the best life of Faraday. It was a success, and Thompson used it later on as a testimonial in his favour to obtain permission from Lord Kelvin to write his biography. Kelvin granted this request at once, and gave Thompson several "sittings," at which scientific topics were discussed, and questions were asked and answered to help him in this work. The Kelvin biography appeared in two volumes in 1910.

In the same year there was published anonymously *Calculus made Easy,* by F. R. S. Thompson wrote this, but the authorship was not disclosed till after his death. The idea of this book was to make the main principles of the Differential Calculus plain to many who had hitherto been rather frightened by it. The book was written in a very amusing colloquial style, and in the Prologue he wrote:

"Being myself a remarkably stupid fellow, I have had to unteach myself the difficulties, and now beg to present to my fellow-fools the parts that are not hard. Master these thoroughly, and the rest will follow. What one fool can do, another can."

Everyone enjoys a "leg-pull," and much wise and clear exposition went with this one. The book was an instant success, and justified completely the somewhat novel and ingenious way of presenting a difficult subject. A few guessed who the author was, but many who should have been able to didn't.

For an account of Professor Thompson's excursions into the scientific literature of the sixteenth and seventeenth centuries, and particularly of his devotion to Gilbert, the father of magnetism, we must refer the reader to the biography.

Two books by Silvanus Thompson deal with his religious faith. The *Quest for Truth* appeared in 1915, and *A Not Impossible Religion* posthumously in 1918. His forbears were Quakers, and he remained a Friend all his life.

A. S. R.

*Can we Set the World in Order?* By C. R. ENOCK, C.E. (Grant Richards, 1916, 3*s*. 6*d*. net.)

Mr. C. R. Enock, a traveller of wide experience and a well-known writer on South America and other tropical lands, has written a most thoughtful and stimulating volume which should be studied by everyone interested in the problems of home and imperial administration. He makes an appeal to the constructive sense of man to see economic problems in their wider bearings, the antithesis of the opportunist policy of the time. Mr. Enock sees the world in a state of chaos, but believes that it is possible to reach conditions of reasonable economic equilibrium, in which the needs of the individual and the community are established in permanent security. Progress so far has been too exclusively measured in terms of natural wealth and pleasure. The result is that the natural resources of the world have been ruthlessly exploited in the race for wealth. This is not only economically unsound, but is not conducive in the long-run to the well-being of all classes and all races. The book must be read for a full understanding of the closely reasoned argument, which is copiously illustrated from the author's wide experience. Briefly, he presses for the need of a science of corporate life which must be constructive rather than observatory. Economics are too academic to get into real touch with realities, and geography, although far more practical, is too deeply imbued with commercialism. Mr. Enock looks to a development of the science of human geography on constructive lines—in other words, the grasp and application of its principles—as a guide to the statesman and administrator.

R. N. R. B.

*The Quantitative Method in Biology*. By PROF. JULIUS MACLEOD (University of Ghent). (Manchester University Press, 15*s*. net.)

In a volume suggesting the school of De Vries' Mutations theory, the writer emphasises the great natural capacity of plant-forms for variation due to inherent *plasticity*; the remarkable changes in many forms under cultivation, according to him, being merely the expression of this natural property, more in some than in others, and thus revealed for the first time. The difference between one species and another is conceived on a purely chemical basis, and is to be estimated on quantitative lines by precise measurement of the range of so-called *primordia*. The latter term, usually used in botany in an entirely different sense, is perhaps unfortunate; it merely expresses what is loosely known as a factor, and the organism is viewed as a collection or *combination* of factors. In Spirogyra, for example, such functions as the length and breadth of a cell-unit are "primordia." The importance of getting adequate data by measurement for every individual factor of an organism, or for a series of allied

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ones, is insisted on, as indispensable for the accurate delimitation of species in systematy; maxima and minima for the primordia being regarded as "specific constants." There is no objection whatever to this being done; the fact that it has not been generally done in the past is merely a question of labour and the immense amount of available material in even a small flora. The field is quite open to anyone who cares to spend his life in the acquisition of such laborious data. What one wants to know, of course, is what good will come of it. Extended examples are given of the possibilities of variation curves, variation "steps," and "gradation curves"; the combination of such data, constituting a quantitative diagnosis of a species, is defined as "Bertillonage"; but the application of the method for the determination of accepted species is not particularly convincing; it being sufficiently obvious that what are termed *species* by a systematist are still empiricisms wholly lacking any precise standard of reference. It is in thus drawing attention to the general rottenness of the foundation of systematy, as also to the fact that the ordinary collector has but the vaguest notion of the range of type in the forms he deals with over only a small area, that the work attains, perhaps, its greatest value. The text is plentifully decorated with italics and headings in large capitals, and under other conditions, possibly, might have been published in German.

*Economics for To-day*. By ALFRED MILNES, M.A. (J. M. Dent & Sons, 3*s*. 6*d*. net.)

With certain reservations we can recommend this book cordially to readers who wish to make a first acquaintance with economics. It is clearly written, and is furnished in many places with good illustrations. It should thus enable a young student or a beginner to obtain a grasp of the outlines of the subject without having a teacher at his side to explain the difficulties of the subject to him. But reservations must be made with regard to the chapter on the Inland Market, in which the author is not so happy in his exposition. Thus there is a point on p. 113 about the demand for money; whether this refers to the quantity theory of money in its crudest form, or to something else, it is difficult to say, and no explanation is offered. Again, about the same place, use is made of three expressions, "demand-price curves," "demand-value curves," and "quantity-value curves," without any indication of the differences between them, if any; which is somewhat confusing, especially as none of these expressions are at all commonly used by economists, let alone general readers. The reference (p. 122) to "various prices" at which demand and supply can balance is also not very clear.

Apart from the ambiguities of this particular chapter, there are one or two slips which should be corrected in a subsequent edition. At present, thanks to the recent epidemic of amalgamations, there are eleven and not eighteen clearing banks (p. 217). Also, it is hardly correct to say that the Bank Charter Act has been suspended four times (p. 220)—it was only in 1857 and in 1914 that notes were issued in excess of the statutory limit. The price of the four-pound loaf in England before the war was nearly sixpence, so that the statement (p. 225) that it sold "for little more than threepence" hardly conveys the correct impression.

In spite of these small defects, Mr. Milnes's book is a welcome addition to the literature of elementary economics, and is adapted to the needs of the general reader as well as those of young students. The publishers are to be congratulated on producing the book in a presentable style at the—to-day—very moderate price of 3*s*. 6*d*. net.

D. K.

*Evening Play-Centres for Children*. By JANET PENROSE TREVELYAN. (Methuen, 5*s*. net.)

This is a very well written book on a subject that is of great interest to everyone who has the welfare of the poorer children of our towns at heart. It should interest particularly social and settlement workers.

It is now twenty-two years since the first play-centre for children was opened at the Passmore Edwards Settlement in Bloomsbury. Mrs. Humphry Ward was the pioneer of the movement, and to her courage and energy and that of her helpers is due its remarkable progress. In this book Mrs. Trevelyan gives a history of the movement, and an account of its principles, objects, and results. From the small beginning, the movement has grown till it now embraces 300 centres in the provinces, and over 30 in London.

These centres, needless to say, have had good influences, not only on the children attending them, but also on their parents.

Vacation schools and organised playgrounds are also described with inside knowledge; and, in appendices, details dealing with the formation of play-centres, and descriptions of nearly a hundred suitable games, are given.

Mrs. Humphry Ward contributes a preface.

P. K. F.

*Meteorology for All*. By D. W. HORNER. (Witherby, 6*s*. net.)

*The Principles of Aerography*. By PROF. A. MCADIE. (Harrap, 21*s*. net.)

Both these books are good ones, for by reading either of them through one gets a very good idea of the subject. Both are informative. By means of facts, tables, diagrams, and photographs, a fund of information is conveyed to the reader. Both are written in an interesting manner, so that the general reader can read right through without being bored.

Mr. Horner's book is written for a wider audience than Prof. McAdie's. It is meant, as the title implies, for everybody. Not for everybody, of course, but for all those who wish to have acquaintance with the methods of forecasting the weather, and who are interested in clouds, snow, lightning, and wind, and the why and wherefore of the phenomena connected with them: who, instead of cursing the "Clerk of the Weather" as a fool, and believing that the prediction of weather is impossible, want to know how far one can predict the vagaries of the weather, and along what lines the solution of the outstanding problems of the science is to be sought. Mr. Horner gives a good

account of what has been done by the meteorologists, and also indicates much that has still to be done.

The instruments used in meteorology, the thermometers, barometers, hygrometers, rain-gauges, sunshine recorders, etc., are well described. It is a pity that the illustrations have not come out better. They are rather small, and some are hardly clear enough; but reproduction of photographs and diagrams, except in an expensive book, is not an easy matter at the present time.

We recommend this book.

. . . . .

Dr. McAdie is the Professor of Meteorology at Harvard University. This book of his is a joy. No one can read it without feeling that Meteorology, or Aerography as it is called here, is a great subject. This is a book for the student—that is to say, a reader is supposed to be equipped with a sound and comprehensive knowledge of elementary Physics; but the general reader may read most of the book without being taken appreciably out of his depth, and as a work of reference on the subject of the weather it is admirable. The importance of this book lies in the fact that the author has put all the latest information on the subject into it, and that it is exceptionally well illustrated. He advocates scrapping the old system of units in force, and bringing in without further ado the C.G.S. system. He believes in classifying clouds according to their origin rather than their appearance. Some of the subjects dealt with, which are not included in the ordinary textbook, are studies of air-flow at different levels, studies of ice-storms, snowfall equivalents, and water-supply, charts for airmen, variations of ocean currents, and recent knowledge of solar phenomena.

A guinea for a work of three hundred pages seems a stiff price, but the book is most beautifully produced in every way. There are more than fifty photographs, and they are all very clear, so too are the diagrams and charts. The man who writes it knows thoroughly what he is talking about, and he knows how to write, so altogether a charming book results.

*The Development of the Atomic Theory.* By A. N. Meldrum, Fellow of the Bombay University. (Oxford University Press, 1*s.* 6*d.* net.)

Dr. Meldrum has given us an interesting contribution to the history of Chemistry. He shows, among other things, that Dalton, who brought out his famous Atomic Theory in 1803, was forestalled by one Higgins by no less than fourteen years. Higgins's ideas about atoms, unfortunately for him, fell on a somewhat heedless world. There is no suggestion in this essay that Dalton plagiarised from Higgins. Both, starting from Newton's doctrine of an elastic fluid, reached essentially the same conclusions, only Higgins the unknown was fourteen years ahead of Dalton.

*Laboratory Manual of Elementary Colloid Chemistry.* By E. Hatschek. (Churchill, 6*s.* 6*d.* net.)

This is a book we can thoroughly recommend. It is, indeed, almost a necessary book for honours students of Chemistry, because some knowledge of colloids is nowa-

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days an indispensable part of a chemist's equipment. This is the first book that has come under our notice which concerns itself exclusively with instructions for carrying out experiments in this branch of Chemistry, and the author is to be congratulated on his work.

An acquaintance with the subject such as may be obtained from the author's *Introduction to the Physics and Chemistry of Colloids*, or from Dr. Lewis's *Physical Chemistry*, vol. i, is presupposed. The experiments described are, generally speaking, the simplest ones, and, wherever possible, those which involve the smallest outlay of apparatus and material. The instructions are clear, simple, and sufficient. We have tried a good many of the experiments described, and find that they work all right.

References to recent literature and to more advanced portions of the subjects touched upon are given at the end of each section. The diagrams are clear, and the form of the book is good. A. S. R.

*Practical Histology*. 3rd Edition. By PROF. J. N. LANGLEY, Sc.D., F.R.S. (Heffer, 10*s*. 6*d*. net.)

*Handbook of Physiology*. 15th Edition. By PROF. W. D. HALLIBURTON, M.D., F.R.S. (John Murray, 18*s*. net.)

It requires no words from us to commend these popular books, but mention may be made that new editions of both, having been called for, are now available. In the *Histology* a number of changes have been made, some due to recent improvements in methods, some to obviate which the experience of the class-room has shown to be difficulties felt by students, and a few to bring descriptions in accord with present nomenclature. In the *Physiology* no fundamental alterations have been found to be necessary. Such as have been made are of the minor kind necessary to bring the work up-to-date.

*The Practical Book of Interior Decoration*. By H. D. EBERLEIN, A. MCCLURE, and E. S. HOLLOWAY. (Lippincott, 35*s*. net.)

This book is quite different from the little volume in leather which one produces from one's pocket in the bus, or at the theatre between the acts. For in weight and girth it is immense, formidable, Falstaffian. But then it costs thirty-five shillings, and it has three authors, and it deals with a big problem. And it is worth the money.

Imagine the three authors taking you, the reader, breathless and excited over the ideal home, showing us (for I should be there too) the ideal furniture and decorations, and explaining to us the why and wherefore of everything ; and then doors are flung open, and beautiful rooms, amply demonstrating the truth of the exposition, are presented to our gaze !

The first part of the book is historical, and describes the decorative traditions of England, Italy, Spain, and France during the last three or four hundred years. An admirable condensation of the subject has been made. The amount of information given is extraordinary. The interest never flags, and the illustrations are well chosen and beautifully produced.

The second part deals with practical decoration and furnishing, discussing such subjects as colour-schemes, floors, walls, pictures, the arrangement of furniture and artificial lighting. The chapter on colour and colour-schemes is perhaps the best in the book, and will be found most useful by every reader.

The third part deals with the problem of mixing styles in decoration and furniture of different periods, and the true path is clearly indicated.

This book is the most useful and comprehensive one on the subject that we have seen.

### OTHER PUBLICATIONS RECEIVED

(*We hope to deal with several of these in future issues.*)

*Intermediate Textbook of Magnetism and Electricity*. By R. W. HUTCHINSON, M.Sc., A.M.I.E.E. (University Tutorial Press, 8*s*. 6*d*. net.)

*Peat Industry Reference Book*. By F. T. GISSING. (Griffin, 7*s*. 6*d*. net.)

*By Nile and Tigris*. By SIR E. A. WALLIS BUDGE, M.A., Litt.D. (John Murray, 2 vols, £3 3*s*. net.)

*Complete Manual of the Auxiliary Language Ido*. (Pitman, 5*s*.)

*Through Deserts and Oases of Central Asia*. By MISS ELLA SYKES and BRIG.-GEN. SIR PERCY SYKES. (Macmillan, 21*s*. net.)

*Fuel Production and Utilisation*. By H. S. TAYLOR, D.Sc. (Baillière, 10*s*. 6*d*. net.)

*Wild Life in Canada*. By CAPT. ANGUS BUCHANAN, M.C. (John Murray, 15*s*. net.)

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